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THE JOURNAL OF THE SOCIETY OF AUTOMOTIVE ENGINEERS

MAY 1922

ANNUAL MEETING DISCUSSION

SOCIETY OF AUTOMOTIVE ENGINEERS INC.
29 WEST 39TH STREET NEW YORK

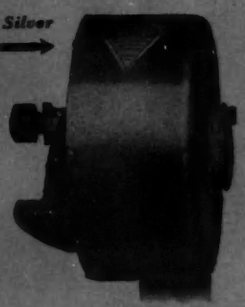
HERE'S A GOOD ONE!

THE other day a Philadelphia automobile man, and he is one of the oldest in the Industry, was arguing that the violence of spring recoil depended entirely upon how ***fast*** the spring had been compressed. He maintained that if a bump were struck in such a way as to ***quickly*** compress the spring, then the recoil would likewise be quick and violent, and that if the spring were ***slowly*** compressed, even though it were compressed to the point of striking the frame, then the recoil would be equally slow and mild.

This same man, and we have encountered others like him, also maintained that spring recoil is like a hammer blow. He could not understand that recoil movement always ***started slowly***. He couldn't get it into his head that spring action and spring re-action being opposite movements, meant that the reactive movement ***had*** to start from zero speed which, of course, means slowly.

When a man can understand that it is not the ***speed*** of spring compression, but the ***extent*** of spring compression which governs the violence of the recoil, and when he can understand that the time to ***control*** spring recoil is not at the tail end of the movement when the movement has become violent, but at the ***beginning*** of the movement before the movement ***gets*** violent, he wants Stabilators because he is in a position to know ***why*** he wants them.

Look for the Silver Name Plate ⟶



Exactly opposite to snubbing

Stabilators do not loaf on the job while the recoil ***force*** is getting the body mass under way—they do not step in during the last half inch when the recoil movement has acquired violence and then try to stop it. Instead, Stabilators are on the job dissipating the recoil ***force*** while the recoil movement is still ***slow***, and hence there is no violent movement to ***require*** stopping. Violent body movement is just what we want to overcome—then why wait until we ***get*** it before we do something about it? The only possible way to control body movement and prevent it from becoming violent is to deal with the force which would cause violence.

We must have regard for the simple laws of physics if we are going to build cars possessing easy riding qualities and good roadability. These laws have not been disregarded in the design and construction of Stabilators.

John Warren Watson Company
Twenty-fourth and Locust Streets
Philadelphia

WATSON STABILATORS

CONQUER ALL ROADS

THE

JOURNAL
OF THE SOCIETY OF AUTOMOTIVE ENGINEERS

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B. B. BACHMAN, *President* COKER F. CLARKSON, *Secretary* C. B. WHITTELSEY, *Treasurer*

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The far reaching scope of the Wyman-Gordon definition of service is exemplified in the location of its two fully equipped forge plants at Worcester Massachusetts and at Harvey Illinois near Chicago. This idea of service is only one of the reasons for the long list of users of Wyman-Gordon Products.

There is no substitute for Wyman-Gordon Quality and Service

WYMAN-GORDON

The CRANKSHAFT MAKERS

WORCESTER, MASS. **CHICAGO, ILL.** **CLEVELAND, OHIO**

THE JOURNAL OF THE SOCIETY OF AUTOMOTIVE ENGINEERS

Vol. X May, 1922 No. 5

Chronicle and Comment

May Council Meeting

THE next meeting of the Council of the Society will be held in Indianapolis on Monday, May 8. A meeting of the Indiana Section is scheduled for the evening of the same day. The Council did not meet during April.

1922 Roster

THE 1922 edition of the Roster of the Society, containing alphabetical lists of the members and of the companies with which they are associated, as well as a section arranged according to the residence of the members, will be ready for delivery within a few weeks. The book will be issued as of April 1, all of the changes of address having been made therein that it has been possible for the office of the Society to secure before that date. Copies of the roster have been ordered by about 2500 members of the Society.

Alcohol for Motor Fuel

THIS month the suggestion of the Research Department of the Society for study by the members is alcohol for motor fuel. On page 364 of this issue of THE JOURNAL a statement of the necessity for meeting the necessary conditions of the utilization of alcohol in automotive products exported from this Country, and a very suggestive if not entirely complete outline of the points of engine design and fuel characteristics involved, are given. The importance of the subject in foreign trade was brought up at the conference convened by Secretary Hoover at the Department of Commerce last month.

Ricardo Paper

MANY members will want to read carefully the paper by Harry R. Ricardo, presented at the January meeting of the Society. This appears on page 305, together with the discussion thereon. The paper has been revised by Mr. Ricardo since it was preprinted, and is, of course, very valuable for study by all interested in features of automotive-engine design and the utilization of currently available fuels therefor. The Council of the Society, on behalf of the members, has transmitted to Mr. Ricardo its sincere thanks in engrossed form for his highly noteworthy contribution to our records.

Summer Meeting

FACTS that the members should know about the meeting of the Society to be held at White Sulphur Springs next month are given on page 345.

Six technical sessions have been arranged and the Meetings Committee has arranged for 16 papers to be presented at them. Reduced transportation rates have been secured from all points to White Sulphur Springs. A well organized Sports Committee is planning a very attractive program and the Entertainment Committee is arranging many events that will appeal to the ladies, as well as the men. At this moment 421 reservations have been received and the choicest accommodations are being taken fast.

Standards Work

A COMPREHENSIVE account of the current standardization work of the Society is given on page 433 of this issue of THE JOURNAL. Brief statements are made with regard to the Division meetings that were held last month, and a tentative schedule of May meetings is given. A number of important subjects, including automobile wiring; ball studs; clutch elements; cotter-pins; battery trays and terminals, charging plugs and receptacles and incandescent lamps for electric vehicles; emergency rim-clamps; felt specifications; front-axle hubs; gasoline-tank openings; generator and starting-motor flange mountings; ignition distributor mountings; involute splines; magneto mountings; motorboat elements; pipe fittings; plate glass; roller chains; and leaf spring elements, are treated, and members interested in any of these matters should read the article carefully.

Part II, 1920 Transactions

IT is expected that Part II of Vol. 15 of the TRANSACTIONS, containing about 800 pages, will be ready for distribution to the members during the latter part of this month. It will be recalled that this volume will be furnished to only those members who have sent a written order for it to the office of the Society, order blanks for the purpose having been published and sent

to the members repeatedly. Less than one-third of the members of the Society have signified their desire that this part of the TRANSACTIONS be sent to them. It includes, in addition to miscellaneous reports, the papers and discussions presented at the 1920 Ottawa Beach Meeting of the Society and at various Sections Meetings, that are considered of sufficient engineering and reference value to be recorded in the Transactions, as well as published in THE JOURNAL.

Employment Service

THE Employment Service conducted by the Society has, it is believed, been much improved recently. The attempt is being made to keep in as close touch as possible with companies needing men and with men seeking positions. An added feature of the service is a Semi-Weekly Bulletin mailed to both companies and men. Employing companies have shown a marked interest in this frequent bulletin service and are selecting many Society members for positions they have available. The companies are not willing in all cases to make public announcement of openings with them. It is therefore advisable that all members desiring positions should send to the office of the Society full information as to their desires and qualifications, in the manner set forth in the notes appearing at the top of the S. A. E. Employment Service column in THE JOURNAL.

Membership Increase

DURING the first four months of 1921, 324 applications for membership in the Society were received. This year, up to April 24, 238 applications were received. The Membership Committee, under the chairmanship of Lon R. Smith, is proceeding actively with the purpose of adding to the membership in a sound way that will be helpful to the industry and the public, as well as the members themselves. Many prominent executives in the automotive industries who have not taken an active part in the Society's work heretofore are showing a decided interest in its affairs. In addition to corresponding with a large number of executives, the Membership Committee is in communication with several hundred men whose names have been submitted by members of the Society on the basis that it is desirable that they be enrolled for the general good.

Highway Research

SEVERAL phases of the highway problem merit discussion by the recently established Highways Committee of the Society, of which the following are members:

H. W. Alden, chairman
W. A. Brush
G. A. Green
W. E. Lay
F. A. Whitten

The Advisory Board on Highway Research of the National Research Council has been active for some time, the object of the board being to center the research facilities of all transport interests on a systematic research program. Much success has been attained in this connection, the organizations represented on the board including all those directly concerned in highway research.

There are many highway problems in which the automotive industry has a vital interest, and some of these cannot be handled adequately without the active assistance of automotive engineers. The effect of vehicles on the road is being investigated by the Bureau of Public Roads and other organizations. The factors in vehicle design, such as weight distribution, springing and tire equipment, are to be studied with the cooperative assistance of automotive engineers. Important items are fuel-consumption, service and depreciation costs as affected by grades and road surfaces. The cost of vehicle operation, including fuel, service and upkeep, is especially important in its relation to cost of highway construction and maintenance. The National Research Council has asked the Society to undertake to develop means for collecting data on the costs of vehicle operation. This information is necessary to establish correctly the economic balance between truck and rail transportation.

Papers at Society Meetings

THE Publication Committee of the Society has called attention recently to certain matters that are of great importance to those who participate in the preparation of and the scheduling of papers for presentation at meetings of the Society and its Sections. The particular point made in this connection is that it is essential that qualitative statements made by authors be supported by quantitative data. Statements made as to the merit of various devices or as to the actuality of alleged facts are, of course, inconclusive unless they are based on authentic test results of a sufficiently comprehensive nature. It is strongly urged that those concerned in the acceptance of papers at Sections Meetings have papers submitted for presentation read in advance by members competent to judge of their merit. Procedure of this kind always tends to bring the best results so far as the time given by members at meetings and the printed record of the Society are concerned.

Financial Affairs

THE net assets of the Society on March 31 amounted to $124,110.89. The corresponding figure on March 31, 1921, was $134,491.61. The difference between the two figures is constituted of the net loss in the operations of the Society during the last 12 months. The net excess of expense over income during the first 6 months of the fiscal year beginning Oct. 1 last was $14,459.60.

The total membership of the Society on March 31 was about the same as on the same day of 1921, those who ceased to be members through non-payment of dues and resignation having offset the number of those newly enrolled. During the first half of the current fiscal year the income of the Society was $13,976.09 less than during the first half of the last fiscal year. Notwithstanding increased expense in the Standards, Research and Employment Service Departments, the total expense of the Society from Oct. 1, 1921, to March 31, 1922, was only a few hundred dollars more than that for the corresponding period of the last fiscal year.

Fuel-Research Program

THE fuel-research program, in which the Society has taken a laboring oar, is making headway, considerable time having been spent recently by the Research Department and officers of the Society on various phases of the whole question.

It is planned that the main research program shall be undertaken shortly at the Bureau of Standards, which has outlined an investigation to estimate the effect of changes in the volatility of gasoline on the average fuel-consumption of the passenger cars now in use. The Na-

(*Concluded on page* 429)

Recent Research Work on the Internal-Combustion Engine

By HARRY R. RICARDO[1]

ANNUAL MEETING PAPER *Illustrated with* CHARTS AND PHOTOGRAPHS

THE author describes the research work on the internal-combustion engine done recently in his laboratory in England, and presents his deductions therefrom, based upon an analysis of the evidence he has obtained to date.

Fuels are discussed at length under three specific headings, many tabular data being included and commented upon, and the calculation of thermal efficiency described. Mean volatility and detonation are discussed and the author's present views regarding turbulence are stated, this being followed by a brief summary of the conclusions reached by Mr. Tizard, a colleague of the author, following recent investigations.

The influence of the nature of the fuel upon detonation is presented, a lengthy discussion of the subject of stratification being given under three specific divisions, inclusive of comment upon the benefits derived from using weak fuel-mixtures. The paper itself is concluded with a discussion of turbulence with reference to combustion-chamber design, many charts and photographs having been included throughout.

The paper is supplemented by nine appendices, which include discussions of mechanical efficiency, under three specific headings; piston experiments, inclusive of four specified deductions; air measurement, with a description of equipment and methods; the total internal energy of the working fluid over a wide range of temperature; the influence of compression-ratio upon power output and efficiency; the influence of cylinder size on performance; the influence of cylinder temperature on power output; the distribution of heat in a high-speed internal-combustion engine; and the efficiency of a single-cylinder engine under reduced loads. This supplementary information is copiously illustrated.

I HAVE read with very great interest the many excellent papers which have been presented to this Society during the last 12 months, and I am deeply impressed by the immense amount of painstaking and careful research that has been carried out in America during this period. Although the experience of the war brought into prominence the great importance of scientific research and gave birth to many schemes and resolutions to encourage and stimulate it, yet the spirit of retrenchment and economy which has pervaded all Europe for the last 18 months has had its inevitable effect and caused the abandonment, or at least the postponement, of many excellent plans. The writer has been singularly fortunate in that, thanks to the encouragement and support of some of the large private British engineering and petroleum interests, he has been able to carry-on with research work through this difficult period and, thanks also to their generous and broad-minded attitude, he is permitted to publish the results obtained.

Last year, Sir Dugald Clerk presented to this Society a paper that I have read with particular interest. In it he has set forth a general summary of the very valuable work he has done. In England, if not throughout the world, Sir Dugald Clerk is very properly regarded as the father of the internal-combustion engine, and I would like to emphasize at the outset that I claim to be no more than a devoted disciple of Sir Dugald Clerk, and to have done no more than to proceed along the lines of research which he has initiated. I also owe a great debt of gratitude to the late Professor Hopkinson both for his teaching and for his stimulating and infectious enthusiasm. To a student of research nothing could be more inspiring than Professor Hopkinson's teaching and personality, for he felt and could impart to others the sheer joy of research work for its own sake; above all, he knew and could teach how to carry out such work quickly, how to separate the grain from the chaff rapidly, and how to reconcile enthusiasm with an open mind.

In the following paper the writer proposes to deal with the work that has been carried out recently at his laboratory, and with the deductions he has drawn therefrom. The deductions are based upon analyses of the evidence he has obtained to date; they represent, therefore, the opinions he holds at the moment and, like all such deductions, are liable to be overthrown or superseded in the light of future work.

FUELS

During the past few years we have been carrying out a good deal of research work on the subject of the influence of different fuels on the performance of an internal-combustion engine. The experiments have all or nearly all been carried out on single-cylinder engines, into which the problems of distribution do not enter. Until, therefore, we have more data on this very intricate subject, I can deal only with the conclusions reached insofar as they are applicable to single-cylinder engines. The three most important factors appear to be the

(1) Tendency of a fuel to detonate
(2) Latent heat of evaporation of the liquid fuel
(3) Mean volatility of the fuel

Broadly speaking, it can be said that all volatile hydrocarbon fuels have the same heat value per cubic inch of mixture giving complete combustion and that, therefore, the power obtainable from all is the same within very close limits when used at the same compression-ratio and at the same volumetric efficiency. The limit of compression-ratio is determined by the tendency of the fuel to detonate; it is a function both of the chemical composition of the fuel and of the maximum flame-temperature. The tendency to detonate by limiting the expansion-ratio controls at once both the power output and the efficiency. The latent heat of evaporation controls the suction temperature, and hence the weight of charge retained in the cylinder; for the same compression-ratio, therefore, the latent heat controls the power output obtainable from any fuel, although the efficiency is, of course, unaffected.

[1] Ricardo & Co., Ltd., London, England.

The mean volatility of the fuel determines the use which can be made of the latent heat of evaporation, for the lower the volatility the more heat must be added externally to the cylinder, and the more the weight of charge will be reduced thereby. It does not appear to be realized generally how important a part the latent heat of evaporation of a liquid fuel plays in the performance of an internal-combustion engine. During the suction stroke of a four-cyle engine, heat is added to the working fluid by

(1) Preheating the carbureter and induction system
(2) Contact with the hot valves and cylinder walls
(3) Admixture with the hot residual exhaust-products in the cylinder clearance-space

Heat is abstracted only by the latent heat of evaporation of the liquid fuel.

For any given aggregate value of (1), (2) and (3), it does not matter whether the fuel is evaporated inside or outside the cylinder, so far as power output is concerned, provided only that the fuel is completely evaporated before the inlet-valve closes. In the former case, the heat applied to the carbureter and induction system will raise the temperature of the air without evaporating the fuel; in the latter, it will evaporate the fuel without raising the temperature of the air. In either case, however, the amount of heat absorbed by the evaporation of the liquid fuel will be the same; and the final suction temperature and therefore the weight of charge retained in the cylinder will also be the same.

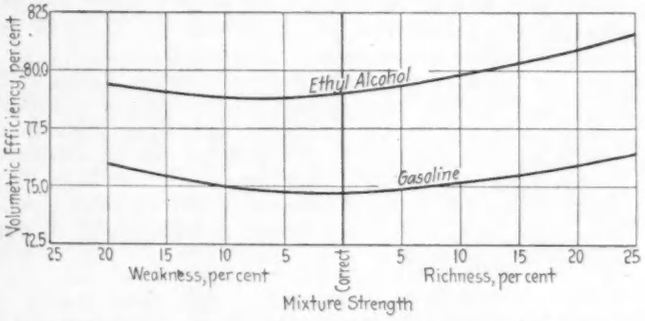


FIG. 1—VOLUMETRIC-EFFICIENCY AND MIXTURE-STRENGTH CURVES FOR A TYPICAL GASOLINE AND NEARLY PURE ETHYL ALCOHOL

So far as pure hydrocarbon fuels are concerned, the latent heat of evaporation is low and does not play a very important part; but, in the case of ethyl alcohol, and still more so in the case of methyl alcohol, the very high power-output obtainable is due almost entirely to the high latent heat of the liquid which is playing an even more important part than is apparent at first sight from a comparison of the mean effective pressure available, because the total internal energy of the alcohols is appreciably lower than in the case of gasoline, benzol and other hydrocarbons.

The curves shown in Fig. 1 give the measured volumetric efficiency at normal temperature and pressure. taken over a wide range of mixture strength with both typical gasoline, (see Fig. 1), and nearly pure ethyl alcohol, each at a compression-ratio of 5 to 1. Table 1 gives the weight of charge per hour, the power output per pound of mixture, the indicated mean effective pressure as found experimentally and the total available internal energy, in the case of three typical fuels, when each is run at the same compression-ratio, with the same amount of preheating and at a mixture-strength giving complete combustion. Column 1 gives the observed weight of charge entering the cylinder per hour at a mixture-strength that permits complete combustion. Column 2 shows the observed indicated horse-power per pound of mixture per hour. Column 3 states the observed indicated mean pressure, at correct-mixture strength, in pounds per square inch. Column 4 presents the calculated heat of combustion of the fuel in terms of foot-pounds per cubic inch at normal temperature and pressure. The engine speed is 1500 r.p.m. and the heat input is 0.0433 B.t.u. per revolution in all cases.

TABLE 1—IDENTICAL TESTS WITH THREE DIFFERENT FUELS

Kind of Fuel	1 Weight of Charge per Cylinder per Hour, lb.	2 Power per Pound of Mixture per Hour, i.hp.	3 Indicated Mean Pressure, lb. per sq. in.	4 Heat of Combustion, ft-lb. per cu. in.
Gasoline	209	0.153	132	48.2
Benzene	212	0.151	132	47.6
99-Per Cent Ethyl Alcohol	224	0.149	138	47.4

It will be seen from Table 1 that, although the power output on ethyl alcohol is greater than on gasoline or on benzene, the power output per pound of charge is actually somewhat lower. I have labored this question of the influence of the latent heat of the fuel upon the power output obtainable because I think it is a very important one in view of the possible use in the near future of certain members of the alcohol group. It does not appear to have been appreciated, nor does the influence of preheating appear to be fully realized. Many designers rely too much on the temperature recorded in the induction-manifold and appear to be under the impression that, so long as the gases are reasonably cool, the power output of an engine is not being penalized; but there can be no greater fallacy. Table 2 gives the indicated mean effective pressure obtained at 1500 r.p.m. from the same group of fuels, with a compression-ratio of 5 to 1, a correct mixture-strength and an engine-room temperature of 15 deg. cent. (59 deg. fahr.). Column 1 is at a constant heat-input of 0.0433 B.t.u. per revolution; Column 2, when preheated to give a temperature in the induction pipe of 25 deg. cent. (77 deg. fahr.); and Column 3, the temperature in the induction pipe with each fuel and with a constant heat-input of 0.0433 B.t.u. per r.p.m. The difference between Columns 1 and 2, in the case of alcohol, is sufficiently striking to need no comment for it will be seen that the effect of raising the temperature from 62 to 77 deg. fahr. results in the case of alcohol in a drop in the mean pressure developed of not less than 19 lb. per sq. in.

TABLE 2—INDICATED MEAN EFFECTIVE PRESSURE AND TEMPERATURE OF INLET GASES

Kind of Fuel	Indicated Mean Effective Pressure 1 lb. per sq. in.	2 lb. per sq. in.	Temperature 3 deg. cent.	deg. fahr.
Gasoline	132	133	26.0	78.80
Benzene	132	128	7.7	45.86
99-Per Cent Ethyl Alcohol	138	119	16.5	61.70

Since the internal energy, and therefore the flame temperature, are substantially the same for all fuels at the same mixture-strength, it follows that the thermal efficiency will be the same. The available range of mixture-strength on the weak side is also substantially the same for all the available volatile liquid fuels; so, in effect, we can say that the thermal efficiency is the same in all cases, with the exception of the members of the alcohol group. These, owing to their lower heat value per gram molecule and higher latent heat, yield a somewhat higher

efficiency because both the compression temperature and the rise of temperature after compression are considerably lower; hence, the direct heat losses and those due to change of specific heat are reduced.

Before proceeding further, I will add a few words as to the method of calculating thermal efficiency. It is generally agreed by engineers and scientists that the useful calorific value of a fuel is its total heat-value less the latent heat of evaporation of the water formed. It is customary, therefore, to reckon the efficiency on the basis of this lower calorific value, and it is legitimate to do so, because the temperature cannot be extended sufficiently in an ordinary cycle to make any use of the heat of condensation of steam.

In determining the heat-value of any liquid fuel in a bomb or other calorimeter, some of the heat of combustion is devoted to overcoming the latent heat of the liquid and is therefore not recorded. In an engine working on the explosion cycle, the entire amount of the heat required to evaporate the liquid is supplied either by the exhaust or by some other source of waste heat; in any event, it is supplied externally to the heat cycle and the actual heat of combustion available is greater than that found by the ordinary methods to the extent of the latent heat of evaporation of the liquid. I contend, therefore,

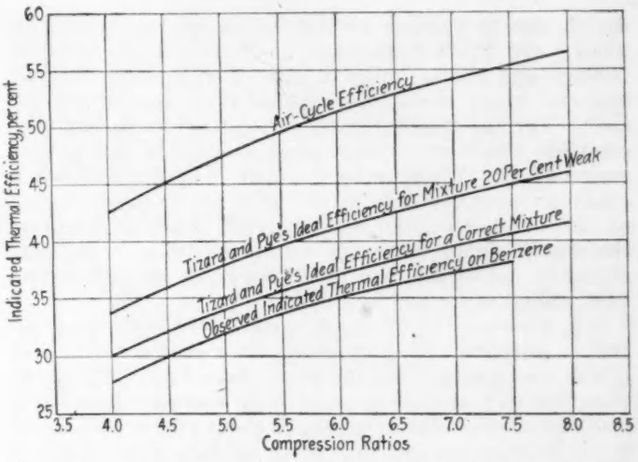

Indicated Thermal Efficiency, per cent
Air-Cycle Efficiency
Tizard and Pye's Ideal Efficiency for Mixture 20 Per Cent Weak
Tizard and Pye's Ideal Efficiency for a Correct Mixture
Observed Indicated Thermal Efficiency on Benzene
Compression Ratios

FIG. 2—INDICATED THERMAL EFFICIENCY FOR VARIOUS COMPRESSION-RATIOS

that if it is legitimate to deduct the latent heat of evaporation of the water formed because it cannot be used, in common fairness the latent heat of the fuel should be added to the calorimeter determination because its equivalent value in the heat of combustion can be and is used in the case of explosion, but not of Diesel, engines. If this be accepted, and I am afraid it must be, then all determinations of thermal efficiency must be reduced. In all the work I have published during the last 2 years the thermal efficiency has been calculated on this amended basis.

Some 2 years ago Messrs. Tizard and Pye completed a very thorough investigation into the limiting efficiency obtainable in a gasoline engine, assuming no loss of heat to the cylinder walls but taking fully into account the losses due to increase in specific heat and to dissociation. They traced very carefully the degree of dissociation and recombination throughout the entire combustion and expansion process, and were thus able to arrive at very nearly the true ideal efficiency for any fuel. Their findings, based upon the latest researches in thermo-chemistry, represent probably by far the most accurate information available at the present date. Briefly they show (*a*) that the efficiency obtainable with any hydrocarbon fuel is the same when burnt at the same compression-ratio, within extraordinarily narrow limits; (*b*) that the ideal efficiency for a chemically correct mixture is as given by the equation $E = 1 - (1/r)^{0.258}$; and that, for a mixture strength 20-per cent weak, it is as given by the equation $E = 1 - (1/r)^{0.295}$.

Fig. 2 and Table 3 give Tizard and Pye's ideal thermal efficiencies for a range of compression from 4 to 1 up to 8 to 1; also, the air-standard efficiency and the observed efficiency.

TABLE 3—TIZARD AND PYE'S IDEAL THERMAL EFFICIENCIES

Compression-Ratio	Air-Cycle Efficiency	Tizard and Pye's Correct Mixture	Tizard and Pye Mixtures, 20-Per Cent Weak	Observed Figure for Benzene, 15-Per Cent Weak	Ratio Observed to Tizard and Pye's 20-Per Cent Weak Mixture, per cent	Ratio Observed to Air-Cycle Efficiency, per cent	Ratio of Tizard and Pye's Ideal 20-Per Cent Weak Mixture to Air-Cycle Efficiency, per cent
4 to 1	42.5	30.0	33.6	27.7	82.5	65.3	79.2
4.5 to 1	45.2	32.2	35.9	30.0	83.5	66.3	79.5
5.0 to 1	47.5	34.0	37.8	32.0	84.5	67.2	79.8
5.5 to 1	49.4	35.6	39.6	33.7	85.0	68.1	80.1
6.0 to 1	51.2	37.0	41.1	35.0	85.3	68.5	80.3
6.5 to 1	52.7	38.3	42.4	36.2	85.4	68.9	80.5
7.0 to 1	54.0	39.5	43.7	37.3	85.4	69.0	80.7
7.5 to 1	55.3	40.6	44.9	38.3	85.2	69.1	81.0
8.0 to 1	56.5	41.6	46.0	...	...	...	81.4

It will be seen from Table 3 that both the theoretical and the observed efficiencies rise with an increase of compression at a greater rate than the air-standard, as the latter is increased and the terminal temperature re-

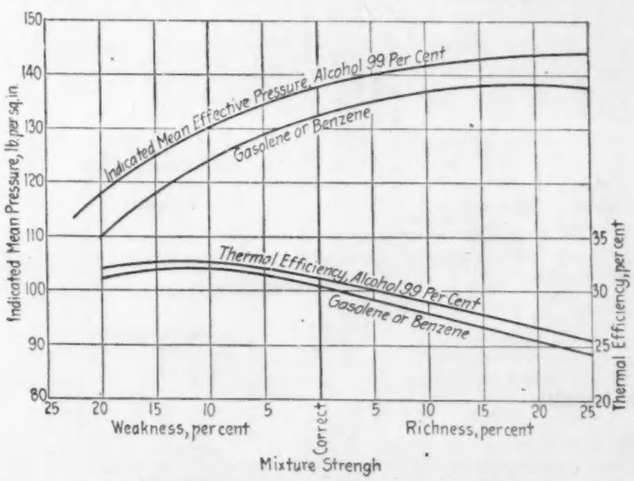

Indicated Mean Pressure, lb. per sq. in.
Indicated Mean Effective Pressure, Alcohol 99 Per Cent
Gasolene or Benzene
Thermal Efficiency, Alcohol 99 Per Cent
Gasolene or Benzene
Thermal Efficiency, per cent
Weakness, per cent
Correct
Richness, per cent
Mixture Strengh

FIG. 3—INDICATED MEAN EFFECTIVE PRESSURE AND THERMAL EFFICIENCY FOR VARIOUS MIXTURE-STRENGTHS OF HEPTANE, BENZENE AND ALCOHOL

duced, due to further recombination during expansion. Tizard and Pye's deductions, as calculated for heptane, benzene and alcohol, cover in effect a wide range of fuels, and the whole available range of mixture-strength for each. For the weakest possible mixture consistent with complete combustion, they have assumed a 20-per cent excess of air. We have been unable to obtain sufficiently rapid or complete combustion with so weak a mixture as this and, in practice, we found that the highest thermal efficiency is always obtained with a 15-per cent excess of air in the case of all the fuels we have examined, except ether and hydrogen.

Fig. 3 shows the observed variation in both mean effective pressure and thermal efficiency over a wide range of mixture-strength for the same three fuels. It will be seen that in each case the maximum thermal efficiency is obtained with a mixture-strength about 15-per cent weak. It will be observed also that, after reaching about a 15-per cent weak mixture, the efficiency falls away due to slow and incomplete combustion. Even with a 15-per cent excess of air, it was found necessary to advance the ignition some 15 deg. earlier, that is, from 32 to 47 deg., than that required for a mixture giving complete combustion. In all these experiments the fuel and air consumption per cycle were recorded automatically and simultaneously, the latter was accomplished by the air-measuring device shown in Figs. 4 and 5. This apparatus is described in Appendix 3.

Mean Volatility

While it would appear that the vapor pressure determines the readiness or tardiness of a fuel to start from a cold condition, it is the mean volatility which decides the amount of preheating required. When considering various fuels, it is desirable to divide them into two groups; those which are homogeneous, such as benzol or alcohol, and those which are heterogeneous, such as gasoline or kerosene. With the homogeneous fuels, comparatively little preheating is required because, although their vapor tension and latent heats may be high, their final boiling-point is low; and, since the object of preheating is primarily to prevent precipitation of the liquid on the walls of the induction system, it is necessary only to raise the temperature of these walls to above the boiling-point of the fuel. When the boiling-point is low, it is possible to raise the whole surface-temperature of the

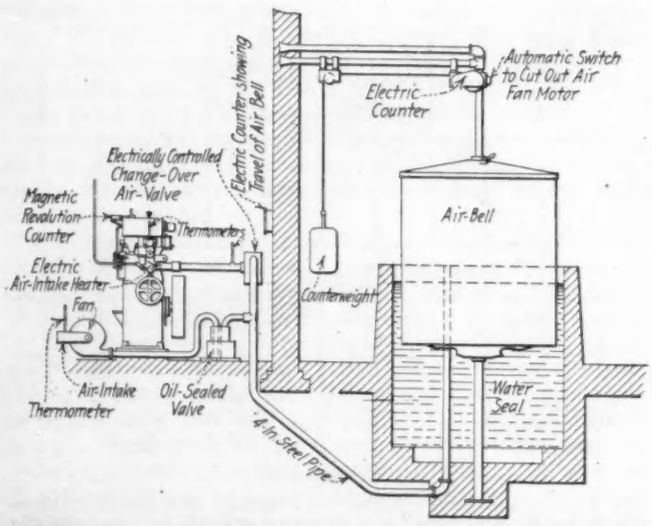


Fig. 4—Diagram Showing the General Arrangement of the Air-Measuring Device

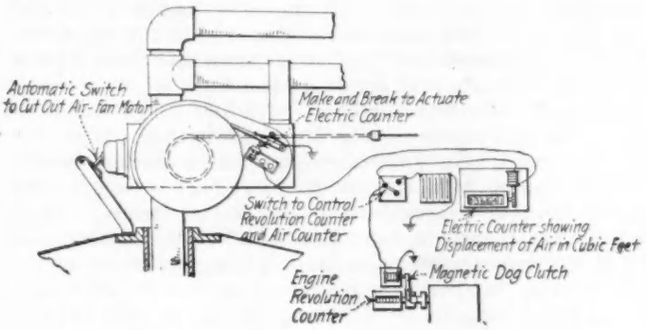

Automatic Switch to Cut Out Air-Fan Motor
Make and Break to Actuate Electric Counter
Switch to Control Revolution Counter and Air Counter
Electric Counter showing Displacement of Air in Cubic Feet
Engine Revolution Counter
Magnetic Dog Clutch

Fig. 5—Electric Counter Used in Connection with the Air-Measuring Device

walls to a sufficient degree without adding much heat to the mixture flowing within.

In the case of fuels having a wide range of boiling-points, such as gasoline, and with a final boiling-point of any value up to 230 deg. cent. (446 deg. fahr.), it is very difficult to prevent precipitation and, at the same time, avoid imparting too much heat to the mixture, for, although the mean volatility of high-boiling-point gasoline and benzol may be, and actually is, nearly the same, the temperature required to prevent precipitation of the heavier fractions is much higher in the case of gasoline, and for the latter only high-temperature heat is of much use. It would appear that in the case of high-boiling-point gasolines it is best to apply a little high-temperature heat just at those points where, owing to changes in velocity or direction in flow, precipitation is most liable to occur, while in the case of the homogeneous fuels low-temperature heat will suffice. There can be no greater mistake than to suppose that, because a fuel's latent heat of evaporation is high and its vapor tension relatively low, it requires excessive preheating.

Vapor tension governs ease of starting but, once started, it probably has very little further influence; thorough pulverization and the avoidance of precipitation then become the most important considerations. This is particularly marked in the case of alcohol which, if properly handled, requires very little preheating; but it does require the most thorough pulverization.

Detonation

The principal factor controlling both the power output and the efficiency of any internal-combustion engine operating on the explosion cycle is the tendency of the fuel to detonate. It is, therefore, not unnatural that so much thought and experimental research have been devoted recently both in the United States and in England to the study of this subject. The phenomenon of detonation appears to be the setting-up in the cylinder of either a true explosion wave or at any rate an extremely rapid rise of pressure. This occurs when the rapidity of combustion of that portion of the working fluid first ignited is such that, by its expansion, it compresses before it the unburnt portion. When the rate of temperature-rise due to compression by the burning portion of the charge exceeds the rate at which it can get rid of its heat by conduction, convection and the like by a certain margin, the remaining portion ignites spontaneously and nearly simultaneously throughout its whole bulk, thus producing a sudden rise of pressure or an explosion wave which actually springs the walls of the cylinder and, reacting in its turn, compresses afresh the portion first ignited. This raises the temperature of that portion further and, with it, the temperature of any isolated or partially in-

sulated objects in its vicinity; thus, it soon gives rise to preignition. Therefore, it would appear fairly certain that detonation depends primarily upon the rate of burning of that portion of the charge first ignited, and it remains to discover what actually controls this rate.

I have always maintained in previous publications that turbulence has an important influence upon the tendency to detonate. This seemed probable, and there was much circumstantial evidence in favor of such an assumption. From more recent investigations, my belief in this has been considerably shaken. I now feel bound to confess that I can find no real evidence that turbulence, while invaluable for other reasons, influences detonation one way or the other. In the case of combustion-chambers designed to give very high turbulence, I found a marked

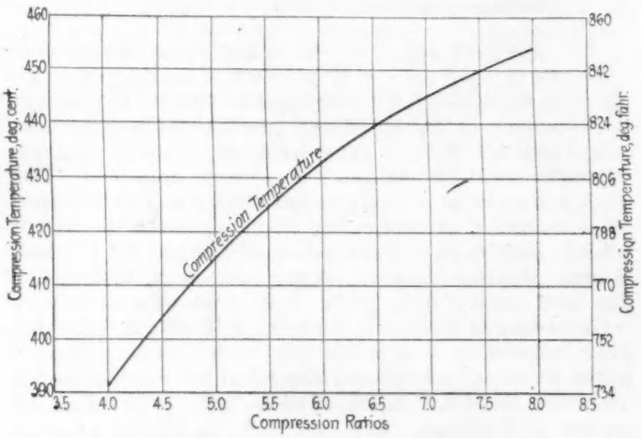


FIG. 6—CURVE SHOWING THE COMPRESSION TEMPERATURE FOR COMPRESSION-RATIOS RANGING FROM 4 TO 1 TO 8 TO 1

reduction in the tendency to detonate, but I am satisfied now that this was due not to the turbulence but rather to the fact that, in each instance, the maximum distance that the flame could travel from the spark-plug was exceptionally small. More recent tests on two engines with multiple valves, in which the turbulence could be varied by cutting out one or more of the inlet-valves, showed that this made no difference whatever as regards detonation.

It was always supposed until recently that detonation is dependent upon the temperature of compression; this appeared plausible enough at first sight, but it most certainly did not fit in with the observed facts. For example, the difference in compression temperature between a compression-ratio of 4 to 1 and 6 to 1 is actually only about 40 deg. cent. (72 deg. fahr.), and this difference can easily be more than counterbalanced by a change in the amount of preheating of the charge; but, we all know from experience that, while detonation on say a reasonably good gasoline will not occur with considerable preheating at a compression-ratio of 4 to 1, it will inevitably occur far below a compression-ratio of 6 to 1 without any preheating at all, and even with stone-cold cylinder-jackets. The curve in Fig. 6 shows to the nearest approximation the compression temperature for a range of compression-ratios from 4 to 1 to 8 to 1, assuming (*a*) the same amount of preheating and the same latent heat in every case, and (*b*) allowing for the varying proportion and temperature of the residual products at each compression-ratio.

Our experiments appeared to show pretty clearly that detonation has very little connection with the temperature of compression, but is closely dependent upon the

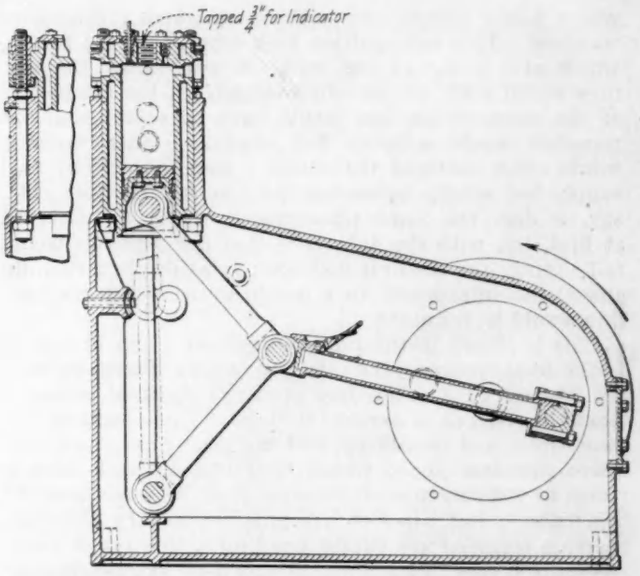


FIG. 7—SECTIONAL ELEVATION OF A MACHINE DEVELOPED BY THE AUTHOR FOR IGNITING FUEL-AIR MIXTURES BY ADIABATIC COMPRESSION

compression pressure. Therefore, I concluded that it is the pressure rather than the temperature of the working fluid which controls the initial rate of burning, and therefore the tendency to detonate. This seemed reasonable. It fitted in nicely with the facts of the case and did duty for a while as an explanation until the chemists objected on the ground that the rate at which combustion takes place under these conditions is generally influenced but little by such relatively small differences of pressure. It then became necessary to cast about for another explanation, and to find one that would satisfy the chemists also.

For the purpose of such an investigation we designed and built a machine for igniting fuel-air mixtures by adiabatic compression alone, and in which either the compression-ratio or the temperature could be varied at will over a wide range. A sectional drawing of this machine is shown in Fig. 7. In this apparatus the piston is operated by a toggle mechanism in such a manner that it makes one rapid stroke and then remains locked at the top dead-center. By varying either the compression-ratio or the temperature of the charge within the cylinder, a condition could soon be found under which any fuel-air mixture would just self-ignite. It was found with this machine that, for every specific fuel and for any mixture-strength of that fuel with air, there

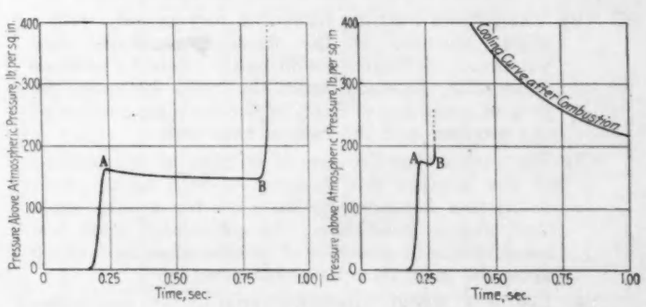


FIG. 8—TWO TYPICAL INDICATOR-DIAGRAMS OF A HEPTANE-AIR MIXTURE OBTAINED WITH THE FUEL SELF-IGNITION MACHINE SHOWN IN FIG. 7

Reading from Left to Right the Maximum Compression Temperatures Were 282 Deg. Cent. (540 Deg. Fahr.) and 307 Deg. Cent. (585 Deg. Fahr.)

was a fairly definite temperature at which self-ignition occurred. This self-ignition took several forms; for example, at x minus 15 deg. cent. (27 deg. fahr.) the mixture would start to burn immediately on the completion of the compression, but would burn so slowly that the pressure would actually fall slightly. Such burning would often continue for nearly 1 sec., the rate of heat supply not wholly balancing the rate of heat loss. At, say, x deg. the same phenomenon would be observed at first, but with the difference that the pressure would fall slightly for the first 0.25 sec.; it would then rush up almost instantaneously to a maximum at which combustion would be complete.

This is shown in the typical diagrams given in Fig. 8. If the temperature were raised a further degree or two, the interval of slow burning at nearly constant pressure would be reduced to perhaps 0.10 sec. Thus, between the most timid and hesitating, and the most prompt and decisive burning, there would be found in each case a range of temperature of perhaps about 30 deg. cent. (54 deg. fahr.); but, since it was possible to vary the compression temperature within fine limits, this range could be explored very thoroughly. It was most extraordinarily consistent and, for any given mixture-strength or any given fuel, one could adjust the machine beforehand with the certainty that one could obtain a 0.10, 0.20, or even a 1.00-sec. interval between the commencement of combustion and the sudden pressure-rise. These first tests were all run with a stagnant charge; later a fan was fitted inside the cylinder and driven at a speed of about 2500 r.p.m. by a small electric motor. With the fan revolving, precisely the same phenomena were observed, but at a slightly higher compression temperature which merely balanced the increased rate of heat loss; the intervals between the commencement of burning and the sudden pressure-rise, although relatively the same, were much smaller in all cases. In short, the fan speeded up the whole process; but it did not alter perceptibly its character.

These investigations have been carried out very recently at our laboratory by Tizard. Since he has not yet had time to prepare his work for publication, it would not be fair for me to enlarge upon it; but he has made public some of the conclusions arrived at, and they have been so amply confirmed, not only by tests on our own research engines but also by their close agreement with general experience, that there seems to be little doubt as to their correctness. Briefly, Tizard's conclusions are that, for any given fuel

(1) Detonation depends primarily upon the rate of burning of that portion of the charge first ignited; in this he confirms the usually accepted theory

(2) The rate of burning increases very rapidly with a slight increase of the flame-temperature; and whether it will prove sufficiently rapid to produce detonation, depends upon the ratio between the rate of evolution of heat by the burning portion of the mixture and the rate of heat loss

(3) The chance that the rate of burning of any portion of the mixture will become so high as to cause detonation depends but little, so far as the practical engine conditions are concerned, upon the temperature or pressure of compression, but rather upon the maximum flame-temperature

(4) For any given mixture-strength, the maximum flame-temperature depends primarily upon the proportion of diluent or exhaust products present. It depends also, of course, upon the compression temperature; but this varies comparatively little over a wide range of compression-ratio, while the variation in the proportion of residual exhaust-products over the same range exerts a much greater influence in diluting and so lowering the temperature of the flame. Thus, a difference of $\pm$ 1 per cent by weight of exhaust diluent will raise or lower the flame-temperature by about 25 deg. cent. (45 deg. fahr.), which is equivalent to a range of compression from 4 to 1 to 5 to 1

(5) If the flame-temperature be reduced by weakening the mixture-strength, a very much higher compression could be used at once. In practice, with the exception of hydrogen, it is not possible to weaken the mixture so as to effect any large reduction in the flame-temperature because, within the narrow range available, weakening the mixture with air results merely in reducing the amount of dissociation without affecting appreciably the flame-temperature

Fig. 9 shows the observed variation in compression-ratio permissible over a wide range of mixture strength. In this experiment the engine was run with wide-open throttle at constant speed and constant temperature, and the compression-ratio was adjusted at each mixture-strength until detonation just became apparent. With both hydrogen as a homogeneous charge and with a stratified charge of aromatic free gasoline, it was found possible to operate at a compression-ratio of about 7 to 1 with a mean mixture-strength 50 per cent weak, and without the least trace of detonation. Fig. 10 shows a similar test with hydrogen, but with the mixture-strength much further reduced, as is possible only with this fuel. On the other hand, with hydrogen the range of burning on the rich side could not be explored because, as soon as any excess of hydrogen was admitted, backfiring occurred through the inlet-valves.

It will be seen that the new theory, although based on much sounder reasoning, squares on the whole with my older theory; and that, in the case of a normal homogeneous mixture, the tendency to detonate depends, in effect, upon the compression pressure, not, as I supposed, because the pressure has any marked influence, but rather because, in any actual engine, the compression pressure is, in itself, a measure of the proportion of inert diluent present in the cylinder. It differs in practice only when, by the use of hydrogen, by stratifying the charge or by the introduction of inert diluents, a weak mean mixture-strength can be used. Tizard's theory has been further confirmed by other tests that we have run, in which

(1) The residual exhaust-products have been cleared away by scavenging with air; it was found then that detonation became severe at once, even with very low compression-pressures

(2) Additional exhaust-products were added, by way of the carbureter; the compression could be raised then to almost any degree, depending upon the quantity admitted

Fig. 11 shows the variation in compression-ratio permissible when, to a gasoline detonating normally at a compression-ratio of 4.85 to 1, varying quantities of additional exhaust-products were added and the compression adjusted in each case until detonation just became apparent. Other tests using nitrogen, steam and carbon dioxide have given similar results. The effectiveness of such inert gases appears to be closely proportional to their specific heats; that is, to their direct influence upon the flame temperature.

Broadly speaking, it would appear from our experiments that two factors determine whether or not a fuel

will detonate. These are (*a*) the self-ignition temperature of the fuel-air mixture and (*b*) the rate of acceleration of burning as the ignition temperature is exceeded.

Both the true self-ignition temperature, if indeed such a term can be used, and the rate of acceleration of burning appear to depend primarily upon the chemical composition of the fuel. As a broad generalization, our experiments confirmed that, as regards groups, the paraffins are the worst offenders from the viewpoint of detonation, the naphthenes are better, the olefins better still, the aromatics next and the alcohols the best of all. This, however, can be regarded only as a very broad generalization, because the individual members of the various groups behave very inconsistently among themselves. Thus, pure pentane will withstand a compression-ratio of 5.85 to 1 and is, in fact, comparable with a naphthene. Pure hexane detonates at a compression-ratio of 5.1 to 1, but pure heptane will barely withstand even a 3.75 to 1 ratio, which is the lowest compression we could reach. The behavior of heptane is particularly striking. The first samples used were obtained from California, from

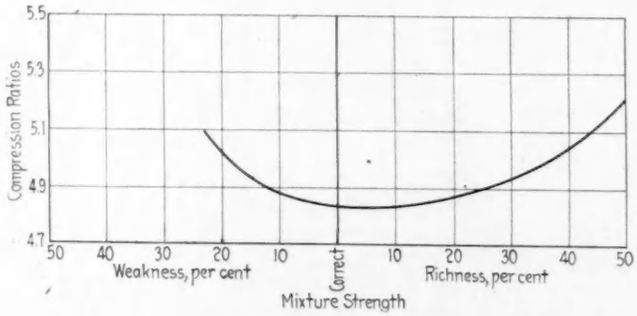


FIG. 9—CURVE SHOWING THE COMPRESSION-RATIO AT WHICH THE DETONATION-POINT BECOMES APPARENT WITH VARYING MIXTURE-STRENGTHS OF AROMATIC FREE GASOLINE AND AIR

a vegetable source; and, although on investigation they were found to be pure samples of normal heptane and not isomeric forms, we still felt some doubt. Later, and with great difficulty, some small quantities of heptane were isolated from aircraft gasoline. Later still, some further supplies were obtained from Germany from Galician oil. Both these later samples gave exactly the same results. It will be seen, therefore, that, while the average of these three members of the paraffin series agrees reasonably closely with the results obtained from a nearly pure light paraffin gasoline, the individual effects appear to be widely different. This difference was not, however, observed when the same three members of the paraffin series were examined in the compression apparatus when their behavior appeared normal. A similar inconsistency was observed in the engine tests in the case of the aromatic series when toluene was found to be markedly superior to xylene and xylene to benzene.

With regard to the rate of acceleration of burning, the case of carbon bisulphide provides a very interesting example. This fuel has a very low ignition-point and will preignite readily at a very low compression-ratio but when added to gasoline it serves as an effective antidetonator and will actually permit a considerable increase in compression. When investigating in the machine Tizard found that, while the rate of burning of most liquid fuels, about the region of their self-ignition temperature, trebles for every 4-per cent increase in temperature, carbon bisulphide required a rise of temperature of 7 per cent to treble its rate of burning. The

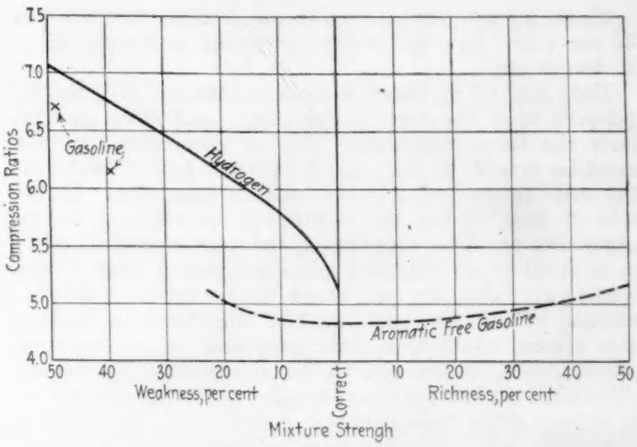


FIG. 10—COMPRESSION-RATIO AT WHICH THE DETONATION-POINT BECOMES APPARENT WITH VARYING MIXTURE-STRENGTHS OF HYDROGEN AND AROMATIC FREE GASOLINE AS WELL AS TWO POINTS OBTAINED WITH A STRATIFIED CHARGE OF AROMATIC FREE GASOLINE

majority of the experiments on which the above conclusions are based were carried out on our 4½ x 8-in. variable-compression research-engine shown in Figs. 12 and 13.

STRATIFICATION

Careful analysis of the behavior of the working fluid in an internal-combustion-engine cylinder shows that under ordinary circumstances we must not look for an efficiency relative to the air cycle of more than about 70 per cent., at ordinary compression-ratios. The principal sources of loss are

(1) Increase of specific heat at high temperatures
(2) Dissociation with incomplete recombination during expansion
(3) Direct loss of heat to the cylinder walls

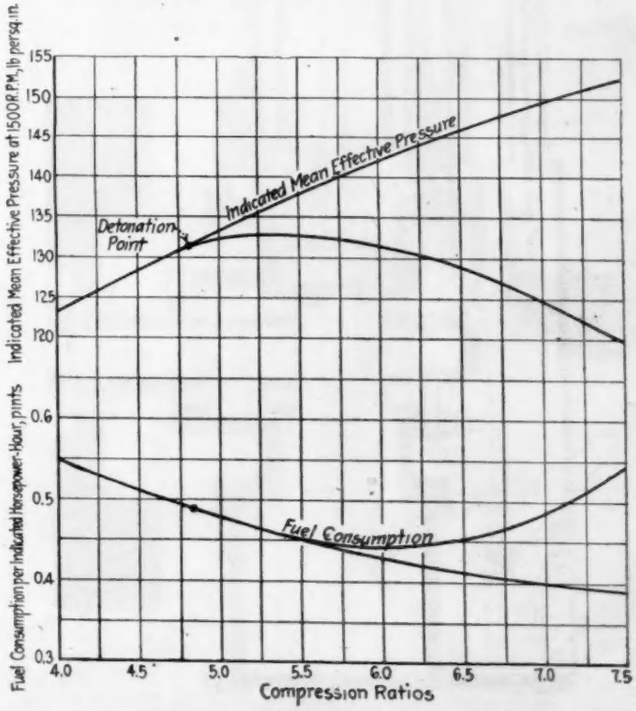


FIG. 11—INDICATED MEAN EFFECTIVE PRESSURE AND FUEL-CONSUMPTION CURVES OBTAINED BY ADDING VARYING QUANTITIES OF ADDITIONAL EXHAUST-PRODUCTS

These amount collectively to an absolute minimum of 30 per cent. In practice they generally approach nearer to 40 per cent.

Each and all of these sources of loss are directly dependent upon the flame-temperature, and more particularly the flame-temperature during combustion, for we must be careful to distinguish between loss of heat during combustion and that lost during expansion. If the loss of heat during the combustion period and before expansion could be suppressed, the heat recovered could be utilized at an efficiency corresponding to that of the whole expansion, or, say, about 40 per cent. A similar amount of heat lost, say, one-half way down the expansion stroke, could have been used only at an efficiency corresponding to that due to the latter half of the expansion; but heat lost toward the end of the expansion stroke is of very little consequence. Perhaps this is rather a digression, but it is customary to refer to the heat lost during combustion and expansion without discriminating between the two.

With any volatile liquid fuel and, with the exception of hydrogen, with such gases as we have examined, the range of burning on the weak side is so narrow that the maximum flame-temperature cannot be reduced to any appreciable extent. Any attempt to do this with a homogeneous mixture results merely in slow and incomplete combustion. Flame lingers in the cylinder, and the fresh charge is ignited on entry. This sets a definite limit to the range that can be used on the weak side, but the practical limit is reached before this when, owing to delayed and incomplete combustion, the thermal efficiency falls off as the mixture-strength is reduced. Fig. 3 shows the variation in thermal efficiency with mixture-strength, over a wide range. Within exceedingly narrow limits, these relations can be said to hold good for any volatile liquid fuel we have ever encountered. It is indeed very

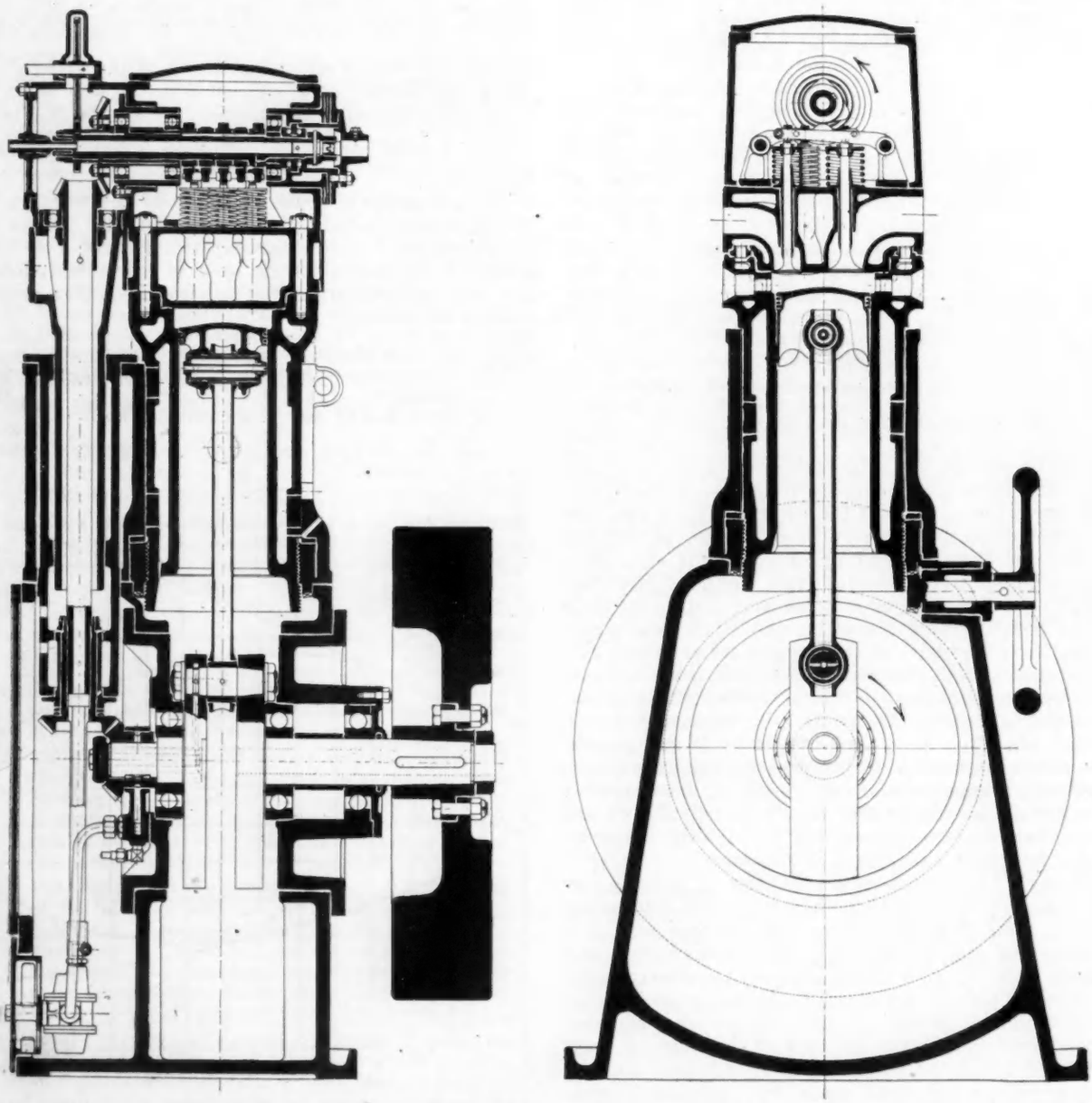


FIG. 12—SECTIONAL ELEVATIONS OF THE 4½ X 3-IN. VARIABLE-COMPRESSION RESEARCH ENGINE UPON WHICH THE MAJORITY OF THE EXPERIMENTS WERE MADE

surprising that, no matter how the chemical or physical characteristics of a fuel may vary in all other respects, they all apparently have this one characteristic in common, namely, that the highest thermal efficiency is always obtained with an excess of air of about 15 per cent, with the one very striking exception of hydrogen. If, by any means, it were possible to work with a weaker mixture and still retain equally rapid and complete combustion, the flame-temperature would fall in relation to the proportion of fuel present.

Benefits of Weak Fuel-Mixtures

It would appear that, if the mixture could be weakened to an unlimited extent, the efficiency would rise from about 70 per cent of the air cycle, with a mixture-

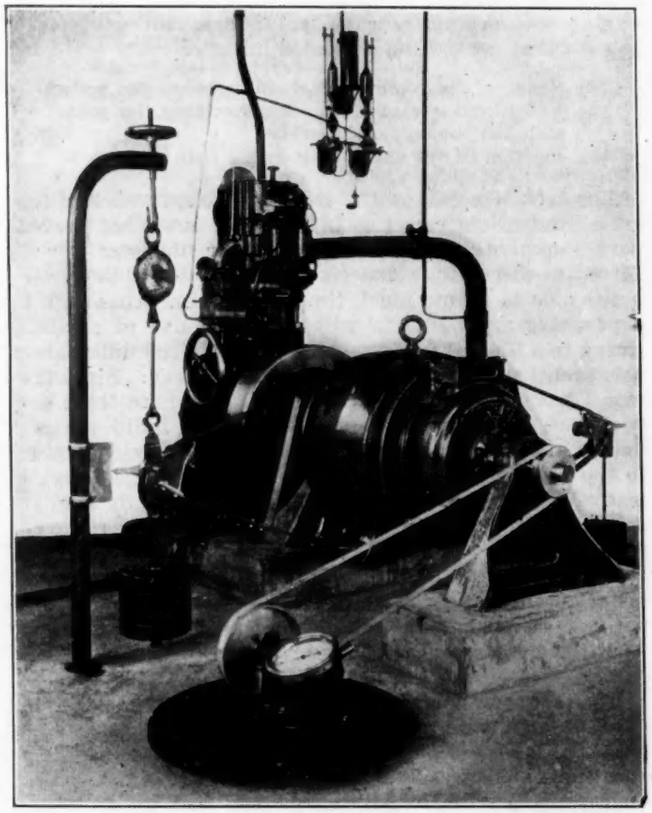


Fig. 13—Exterior of the Variable-Compression Research-Engine

strength 15 per cent weak, in a nearly straight line until, at the point of no heat supply, it would become coincident with the air-cycle efficiency, much as is shown in Fig. 14. But this is not all, for, as the mixture-strength and therefore the flame-temperature are reduced, so also is the tendency to detonate; hence, if means could be found for extending the range of combustion on the weak side, and an engine were designed to use always a somewhat weak mixture, a very much higher compression-ratio could be employed.

The writer has always believed that the benefits of working with a weak mixture could be realized by the employment of stratification; that is to say, by using a little relatively rich mixture and a large proportion of pure air, and keeping the two separate until after the ignition of the former. He has succeeded in doing this experimentally, and in approaching reasonably nearly the results which theory would lead one to expect. In fact,

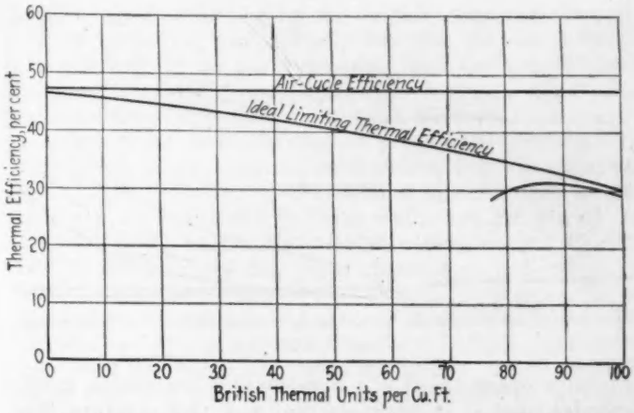


Fig. 14—Curves Showing Relation between the Theoretical Thermal Efficiency and the Energy Content of the Mixture

he did this so easily and obtained such excellent results at the first attempt that he was misled into supposing that he could apply it to existing engines. Subsequent bitter experience has shown that he had quite an unusual measure of good luck in his first attempt. Fig. 15 shows a sectional drawing of the cylinder-head used for the first experiments. It consists of a conical combustion-chamber in which the air-inlet and exhaust valves are fitted, and a small pocket connected with the main combustion-chamber through a narrow neck. An automatic inlet-valve for the admission of a little fuel and air, and the igniter, are fitted in the pocket. The main air-inlet valve is set to open rather late. The result is that, as the piston descends during the suction stroke, the automatic inlet-valve in the pocket or bulb opens first, and air and fuel are drawn in through a throttle-valve from what may be termed the simplest possible expression of a carbureter, that is, a plain uncompensated jet in a choke tube. About 20 per cent down the stroke, the main air-valve is opened and thereafter pure cold air enters the cylinder below the pocket. At the time when the main

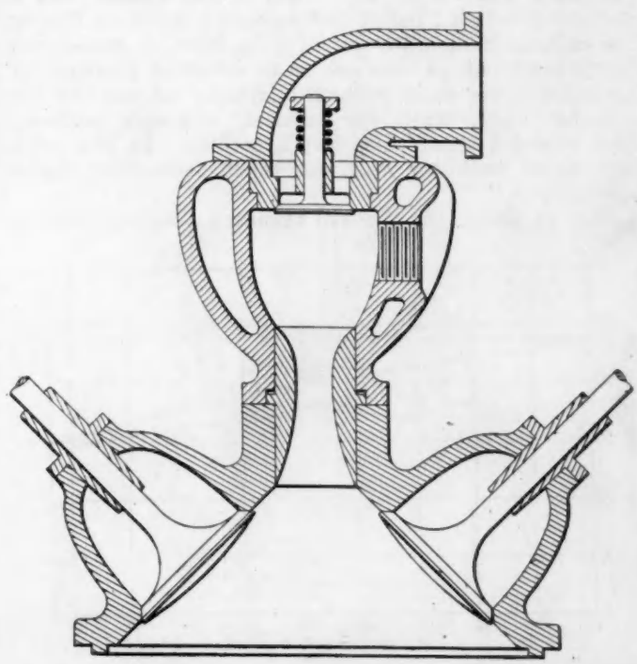


Fig. 15—Sectional Elevation of the Cylinder-Head of the Stratified Charge Engine

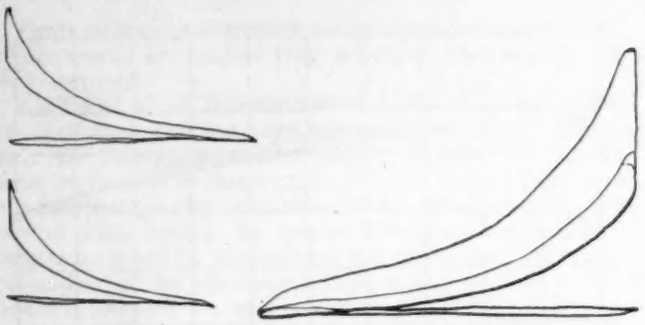


FIG. 16—THREE INDICATOR-CARDS OBTAINED WITH THE STRATIFIED-CHARGE ENGINE

air-valve opens there is a considerable depression in the cylinder, and it is probable that any rich mixture that has overflowed from the pocket retires back into it when the depression is relieved. On the compression stroke, the contents of the cylinder are compressed and some air is driven into the bulb, creating great turbulence therein. The mixture in the bulb is fired at the end of compression and, owing to the turbulence, it burns with extraordinary rapidity. The flaming mixture then rushes out through the narrow neck where it meets and immediately mixes with a very large excess of air; thus, by igniting a small proportion of rich mixture in the first place, I succeeded, in effect, in igniting a large volume of very weak mean-mixture-strength. The power output of this engine could be varied, either by the quantity of fuel alone, or by varying the proportion of mixture of nearly constant strength admitted to the bulb.

With either method of control, the engine would run at any load from dead light to an indicated mean effective pressure of about 120 lb. per sq. in.; but, to obtain the best thermal efficiency throughout so wide a range, it was found necessary to combine both methods. All this seemed beautifully simple and wonderfully effective, and the running of the engine on the light loads was almost exactly like that of a steam engine in its smoothness and sweetness generally. The experimental engine had a water-cooled bulb; but it had no water-jacket or fins on the cylinder and, although of 5-in. bore, it would run continuously at an average mean effective pressure of about 20 lb. per sq. in. without blistering the paint on the cylinder walls, while the exhaust, although perfectly free, sounded almost like deep breathing. In Fig. 16 a number of indicator diagrams taken from this engine are shown.

Fig. 17 shows the thermal efficiency obtained from it

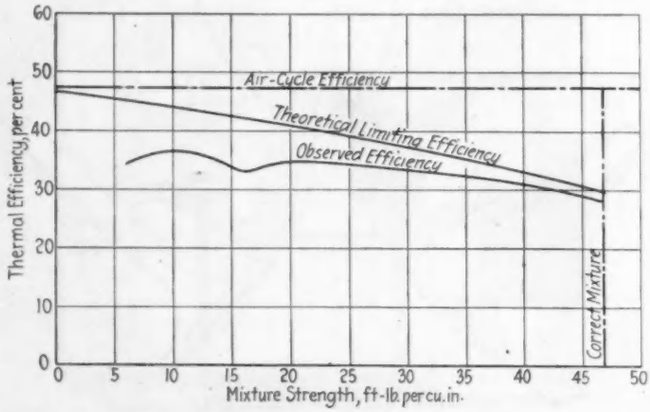


FIG. 17—EFFICIENCY-MIXTURE-STRENGTH CURVES OBTAINED FROM THE TESTS ON THIS ENGINE

with a compression-ratio of 4.8 to 1 and when running at a speed of 600 r.p.m. So far, everything seemed satisfactory and, indeed, so far as this one engine was concerned, it certainly was satisfactory, the only defect being that, do what I would, I could not avoid a small zone of relatively low efficiency over some part of the range. I could vary the position of that zone up and down the range, but I could not eliminate it. Next, I tried the experiment of fitting similar bulbs to several engines with different types of combustion-head, merely substituting the bulb in place of the spark-plug. This proved a disastrous failure in every case. At certain loads, generally at about one-quarter load, I could obtain a very high efficiency indeed; but this was counterbalanced by a miserably low efficiency and irregular running at other loads and, in most cases, I failed utterly to carry more than from one-half to two-thirds the load of which the engine was ordinarily capable. Subsequent experience showed that everything depended upon the

(1) Shape of the combustion-chamber below the pocket
(2) Shape and size of the neck connecting the pocket with the combustion-chamber
(3) Position of the spark-plug in the bulb

The dependence upon (3) is remarkable, in view of the excessive turbulence set up in the bulb; but it has proved very sensitive, none the less. During the war I was forced to discontinue experiments along these lines, but I was able to devote much time to experimenting with a supercharging engine in which I made use of stratification to a limited extent. After many initial difficulties, this eventually proved to be very satisfactory. Since the war I have had little opportunity to return to these experiments. However, some 2 years ago, I did convert one of the two high-speed gas-engines supplying power to the laboratory, to run with a stratified charge, but I was careful to fit a cylinder-head and bulb exactly similar to that which I used on the first engine. This has proved to be entirely successful after about 20 months of continuous running. The engine runs in parallel with a sister engine similar in every other respect, but operating with a homogeneous mixture and throttle-governed. There is no comparison between the two in general behavior.

Fig. 18 shows a sectional drawing and an elevation of this gas engine. On the other hand, the engine using a homogeneous charge requires a top overhaul and the exhaust-valve reseated every 2 months. The engine using a stratified charge has had its cylinder-head removed only once in 20 months. The exhaust-valve then was found to be in excellent condition. There was no carbon on the piston, there being only a little coating of thick oil of the consistency of tar that could be wiped off with a rag, thus leaving the piston clean and bright. When running in parallel on a fluctuating load, but with an average load-factor of about 50 to 60 per cent, the governing on the gas supply alone has proved perfectly satisfactory. The only defect is that, while its sister engine will develop a maximum brake mean effective pressure of 110 lb. per sq. in., the engine using a stratified charge will not do more than about 80 lb. per sq. in., showing that it has not the overload capacity of its sister engine. Incidentally, it is interesting to note that this engine will run also without a trace of detonation on aromatic free gasoline up to a mean effective pressure of about 80 lb. per sq. in., despite the fact that its compression-ratio is 6.5 to 1, thus supplying additional evidence in favor of Tizard's theory about detonation.

As a result of these experiences, the writer believes there is little doubt that, sooner or later, the system of working with a stratified charge will become commercial, for, as he has shown, it is possible and the high efficiency theoretically obtainable from it can be approached. The worst feature about it is that, if not just right, it may be very wrong; a very small change in form or dimension may upset the whole system. In such a case the thermal efficiency may very easily be much worse than that of a throttle-governed engine. It will not be a commercial proposition, he fears, until the influence of each of the variable factors has been determined separately and evaluated correctly.

Turbulence and Combustion-Chamber Design

The great value of turbulence lies, in my opinion, in the

(1) Speeding-up of the process of combustion by the mechanical distribution of flame

(2) Scouring effect it produces in washing away the stagnant layer of gas adhering to the cylinder walls

It appears now to be clearly established that the normal rate of burning of a fuel-air mixture is, at the start, far too slow to be of any value; although, as shown by our experiments on the adiabatic-compression machine, its acceleration after a certain stage is extremely rapid. It is probably in speeding-up the initial stages of combustion by spreading mechanically the pale and timid flame brought into life by the spark-plug, that turbulence plays its most important part, and this becomes most conspicuous when operating with weak mixtures.

Again, there is a good deal of presumptive evidence to show, that, in any combustion-chamber, there is always a layer of gas adhering to the walls of the chamber which, owing to its proximity to these walls, can get rid of its heat so rapidly that it does not burn completely. The effective thickness of this layer depends very largely, no doubt, upon the degree of turbulence. It appears to be this factor rather than the greater direct heat-loss which renders certain forms of combustion-chamber commonly used with side-valve engines so inefficient. There is much to be said in support of this theory, because it will help to explain the very marked difference in efficiency and power as between a high and a low degree of turbulence in the same combustion-chamber, even when using relatively rich and prompt-burning mixtures and firing from two points. Also, while the efficiency obtainable from two forms of combustion-chamber of much the same surface-area may be very different, the heat loss to the cooling water is generally much the same in both cases, showing that it is in the proportion of heat rejected to the exhaust that the difference in efficiency appears to lie rather than in that lost to the cylinder walls.

In his excellent paper on Turbulence,[2] H. L. Horning refers to and illustrates a form of combustion-chamber

[2] See The Journal, June, 1921, pp. 584 and 585.

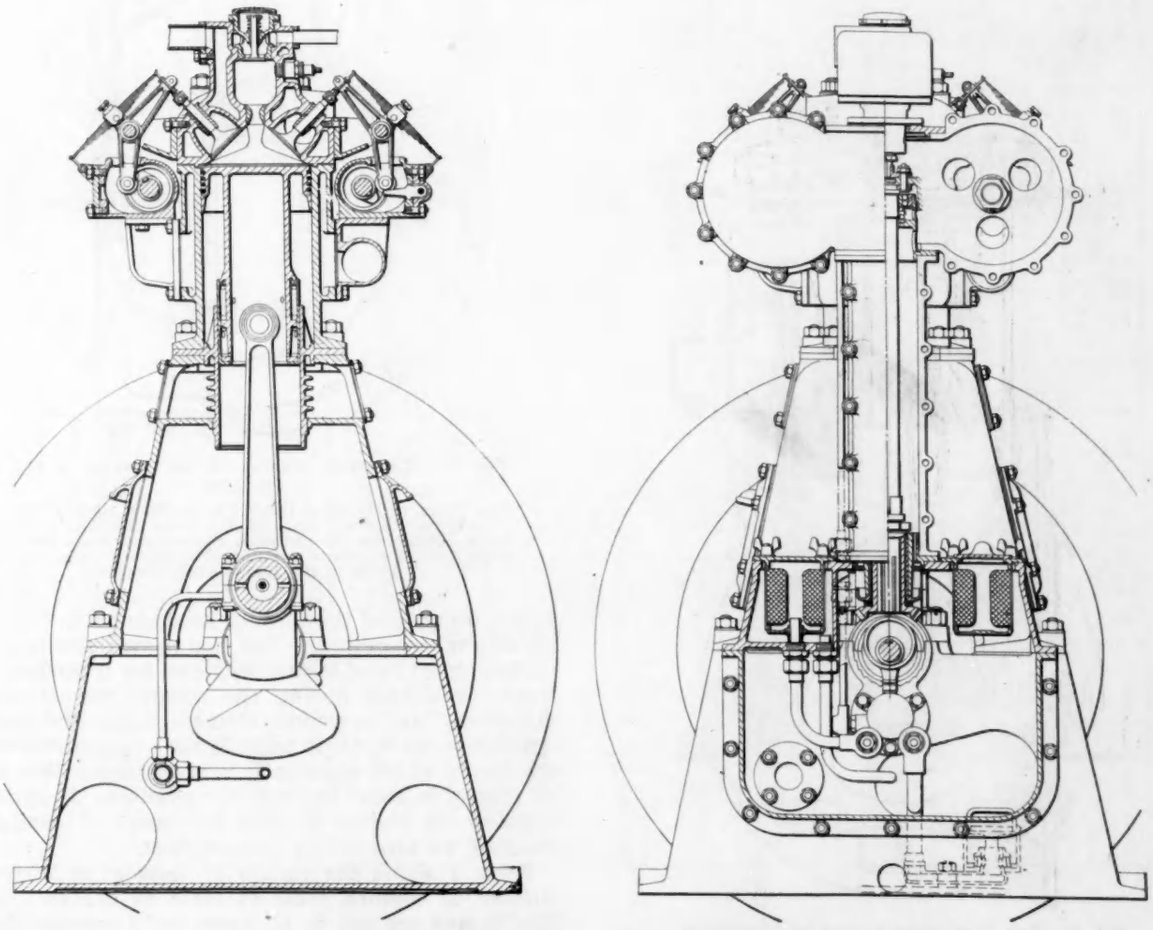


Fig. 18—Elevations, Partly in Section, of the Stratified Charge Gas Engine

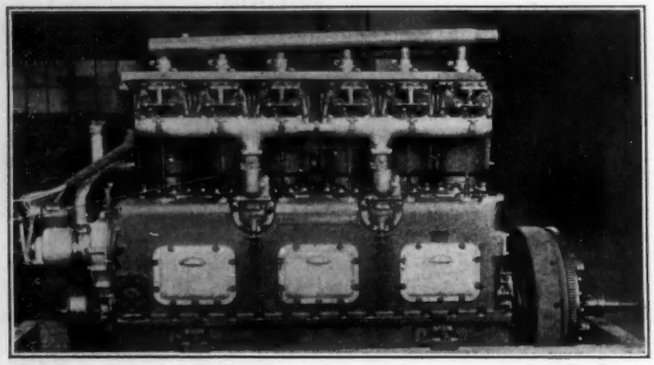


FIG. 19—EXTERIOR OF THE 225-HP. RICARDO TANK-ENGINE

that we have used in the small engines we designed for Mark Webber, in England, and also in all the standard 150-hp. tank-engines. It is shown in Fig. 11 of Mr. Horning's paper and he remarks that, in his experience, this form of chamber with the spark-plug over the piston is unsatisfactory from the viewpoint of detonation. I can endorse this statement fully. We have found this form of combustion-chamber remarkably efficient from the viewpoint of power output and efficiency, for, with a compression-ratio of only 4.3 to 1 we were able to obtain, both in the Mark Webber and in the tank engines, a brake mean effective pressure of 108 lb. per sq. in. and a fuel-consumption of only 0.55 lb. of gasoline per b.hp-hr. in the Mark Webber single-cylinder and of 0.57 in the six-cylinder tank-engines. But we always found it bad from the standpoint of detonation; so bad that in the larger and later tank engines we decided very reluctantly to abandon it in favor of a much more compact form using horizontal valves, and with the spark-plug in the center, as shown in Figs. 19 and 20. This gave about equal results as regards efficiency and mean effective pressure, but was remarkable as regards its freedom from detonation; in fact, these large engines would run comfortably even on kerosene with a compression-ratio of 4.3 to 1 and without any cooled exhaust-products.

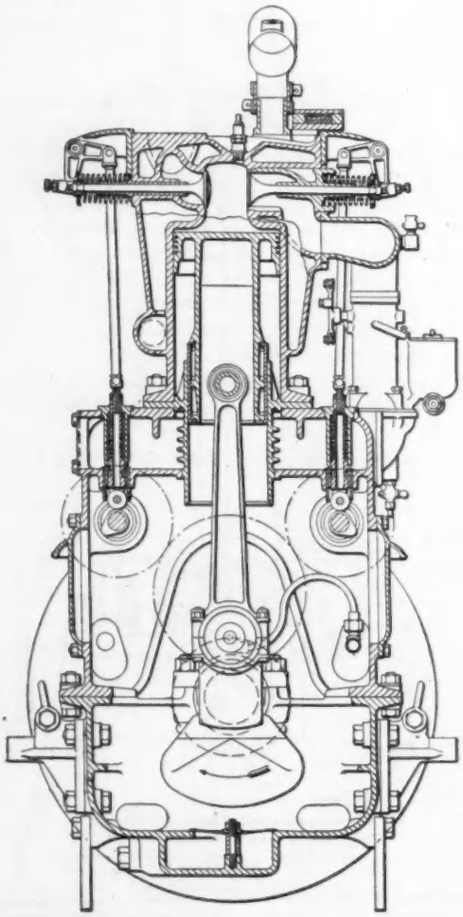


FIG. 20—END ELEVATION, PARTLY IN SECTION, OF THE 225-HP. RICARDO TANK-ENGINE

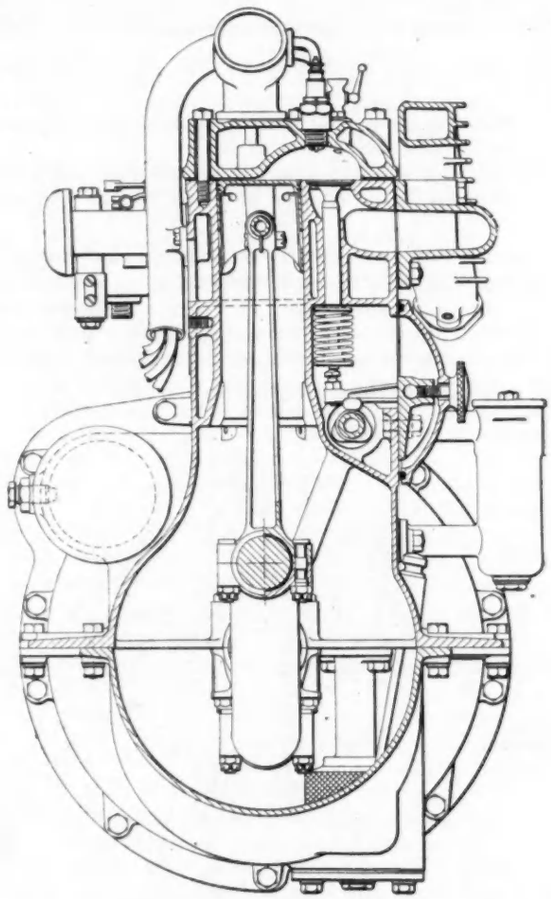


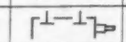
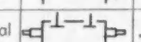
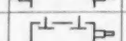
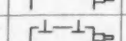
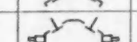
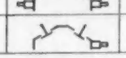
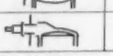
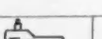
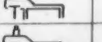
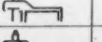
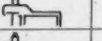
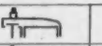
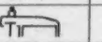
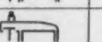
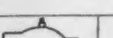
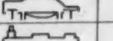
FIG. 21—ELEVATION, PARTLY IN SECTION, OF A 11.9-HP. ENGINE

The Form of Cylinder-Head Shown Was Adopted for All Side-Valve Engines Since It Produced Additional Turbulence during the Compression Stroke and Also the Minimum Distance from the Point of Ignition to the Farthest Point in the Combustion-Chamber

Later, we adopted the form of head shown in Fig. 21 for all side-valve engines. This has proved the best of all we have tried for side-valve engines, for it produces additional turbulence during the compression stroke and, moreover, the maximum distance from the point of ignition to the farthest point in the combustion-chamber, the factor which apparently controls detonation beyond all others, is about the smallest possible. It appears to combine the virtues of both the forms of combustion-chamber we used in the tank engines.

Table 4 shows the results of tests of a fairly large number of engines, most of them of our own design. The figures are not in all cases truly comparative but they probably approach a true comparison as nearly as

TABLE 4—RESULTS OF TESTS ON VARIOUS TYPES OF ENGINE

Column No.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	
Type	Type of Cylinder Head	Form of Head and Position of Spark-Plugs	Valves per Cylinder	Cylinders	Volume of Cylinder, cu. in.	Compression-Ratio	Speed, r.p.m.	Brake Mean Pressure, lb. per sq. in.	Mechanical Efficiency, per cent	Indicated Mean Effective Pressure, lb. per sq. in.	Indicated Mean Effective Pressure Corrected for R = 5, lb. per sq. in.	Indicated Mean Effective Pressure Corrected for 100 cu. in. of Cylinder Volume lb. per sq. in.	Indicated Thermal Efficiency Corrected for R = 5, per cent	Relative Efficiency, per cent	Remarks
Stationary *	Plain Cylindrical		2	1	28.95	4.45	1200	110	84.5	130.0	135.5	137.5	30.0	63.3	Slight Tendency to Detonate
Experimental Stationary *	Plain Cylindrical		5	1	126.90	5.00	1750	121	86.5	140.0	140.0	139.5	32.0	67.5	
Automobile *	Plain Cylindrical		2	2	30.80	4.80	1750	114	83.0	137.5	140.0	142.0	30.4	64.0	
Aeronautic *	Plain Cylindrical		2	6	106.70	4.70	1400	121	87.0	139.0	143.0	143.0	27.3	57.5	
Racing Motorcycle *	Pent Roof		4	1	30.40	5.40	3000	121	87.5	139.0	135.0	137.0			No Detonation under any Circumstances
Experimental *	Pent Roof		4	1	551.00	4.85	1250	135	88.0	153.0	155.0	143.0	33.0	69.5	Slight Tendency to Detonate
Aeronautic *	Pent Roof		4	12	72.60	4.80	2200	120	88.0	136.5	138.5	139.0	27.7	58.3	
Experimental *	T-Head		2	1	83.00	5.40	1500	114	87.0	131.0	127.0	127.0	31.4	66.2	Excessive Detonation, could only be run on Benzol
Tank *	T-Head		2	6	175.50	4.30	1200	106	87.0	122.0	129.0	127.0	31.1	65.5	Severe Detonation
Motor Truck *	T-Head		2	4	101.20	4.70	1200	102	83.0	123.0	126.0	127.0	29.2	61.5	Severe Detonation
Automobile *	Special Side Valve		2	4	35.40	5.00	1800	107	88.0	122.0	122.0	124.0	28.1	59.2	Very Little Detonation
Motor Truck *	Special Side Valve		2	4	99.20	4.70	1100	104	88.0	118.0	121.5	121.5	28.3	59.6	
Automobile *	Special Side Valve		2	4	23.80	4.80	2000	96	86.0	112.0	114.5	117.5	29.9	63.0	
Automobile *	Special Side Valve		2	4	31.70	5.00	1800	109	86.0	127.0	127.0	128.5	31.1	65.5	
Automobile	Ordinary Type Side Valve		2	4	35.40	5.05	1600	96	87.0	110.0	110.0	112.0	24.2	51.0	Severe Detonation
Automobile	Ordinary Type Side Valve		2	4	99.20	3.40	1000	86	86.0	100.0	114.0	114.0	26.9	56.6	
Automobile	Ordinary Type Side Valve		2	4	23.80	4.80	2000	90	86.0	105.0	110.0	113.0	26.9	56.6	
Automobile	Ordinary Type Side Valve		2	4	50.30	4.00	1750	92	87.0	106.0	115.0	116.0	24.9	52.5	
Motor Truck *	T-Head		2	4	94.60	4.25	1400	94	90.5	104.0	112.0	112.0	26.4	56.6	No Detonation
Motor Truck	T-Head		2	4	95.20	4.00	1200	76	81.0	94.0	102.0	102.0	25.3	53.3	Severe Detonation

any that can be obtained, short of taking an experimental cylinder and grafting different combustion-chambers and valve-gears upon it, which would be no easy problem. In all cases marked with an asterisk the valve-opening diagram is the same and so is the gas velocity throughout the induction system, because we have arrived at a more or less standard timing and valve-opening diagram for all of our engines. Also, in all cases an engine speed has been chosen corresponding to a mean velocity through the valves of from 130 to 150 ft. per sec.

From this we may presume that the volumetric efficiency is substantially the same in all cases. The fuel used for the tests is Borneo gasoline in each instance but, unfortunately, the heat-input to the carbureter could not be measured in most cases, and introduces an indeterminate variable. The figures in column 12 represent, to the nearest approximation, the true figure of merit for the particular combustion-chamber. As regards columns 13 and 14, it must be borne in mind that, except in the case of single-cylinder engines, irregularities in distribution may easily more than outweigh any differences due to combustion-chamber design, as discussed in Appendix 3.

Since, within very close limits, the volumetric efficiency can be presumed to be the same in all cases marked with an asterisk, column 2 gives the nearest approach to the true comparative efficiency of the different forms of combustion-chamber, but it is necessary to remember that valves which open directly into the cylinder-head have a somewhat higher orifice-coefficient than those which open into side pockets and that, in consequence, the volumetric efficiency will be somewhat higher in the case of the overhead-valve engine, although, in all cases, ample area was provided around the valve-heads. It will be seen from Table 4 that the overhead-valve engines, whether of plain cylindrical form or of the pent-roof type, appear each to be about equally good.

The type described as an L-head, with a modified valve arrangement in which the inlet is located over the exhaust, appears to come next in order of merit, while those fitted with the design of combustion-chamber shown in Fig. 21, apparently come third. It must be noted, however, that the so-called L-head combustion-chamber is very bad from the viewpoint of detonation, so that it necessitates the use of a lower compression-ratio than that employed in the next group. It will be found that the order of merit is reversed when this is taken into account. The remarks as to the tendency to detonate must be understood to refer to this tendency relative to the compression-ratio employed.

In conclusion, I wish to express my thanks to all the members of my staff for their whole-hearted cooperation and assistance.

FIG. 22—VIEW OF THE PISTON-TESTING APPARATUS EMPLOYED BY THE AUTHOR

APPENDIX 1

MECHANICAL EFFICIENCY

In previous papers, I have given both the results of my experiments on mechanical efficiency and the deductions drawn therefrom. These can be summarized generally by stating that piston friction accounts always for more than one-half of the total mechanical losses and in many cases for nearly three-fourths. It appears to be controlled by three factors,

(1) Area of rubbing surface
(2) Average pressure between the surfaces; in high-speed engines this is due largely to the resolved component of the inertia forces
(3) Condition of the lubrication

The means of dealing with the first two factors are obvious; namely a reduction of the surface and weight. Fortunately, a reduction in the weight results in a reduction also in the pressure on the surfaces due to inertia, and so renders smaller bearing surfaces permissible. The partial carbonizing and congealing of the lubricating oil is more difficult to deal with, for, toward the end of the expansion stroke, a very large proportion of the surface of the barrel, and of the oil adhering to it, is exposed to flame at a temperature of about 2000 deg. cent. (3632 deg. fahr.), and it is by no means easy to see how this can be prevented.

In all of our tests for determining mechanical efficiency by "motoring," which consists in driving the engine by the motor used for the dynamometer and measuring the power required electrically, we find that if the ignition is switched off and the engine turned over to motoring, immediately after running on full load, the total friction is at first rather high but gradually drops until, after a period of say 15 min., it has fallen at least 10 per cent and often as much as 20 per cent due, presumably, to the gradual change from fouled to clean oil on the cylinder walls, and this without any change in temperature.

Although skeptical at first, I have lately come to the

conclusion that the motoring test, if carefully carried out under conditions approaching as nearly as possible the actual running conditions, in most cases gives a surprisingly accurate measurement of the true friction-losses, despite the fact that many conditions are changed. It should be taken into account that, when motoring, the fluid pumping losses are, on an average, some 50 per cent higher than when running normally because (*a*) the exhaust back-pressure is much greater owing to the fact that there is no high pressure at the time of discharge to supply the kinetic energy necessary to propel the exhaust products down the exhaust-pipe, and (*b*) there is a loss of heat during the compression-stroke which is not fully recovered during expansion, with the result that there is a negative loop to the compression-expansion card.

This increase in fluid pumping losses, however, nearly balances, on an average, the decrease in piston friction when motoring and this appears to be true over a wide range of speed. Another method, which we always use as a check-test for mechanical losses in multi-cylinder engines, is to run the engine "all-out" with all cylinders firing until all conditions have steadied down; then short-circuit the ignition of one cylinder at a time, recording accurately the power developed in each case by the remaining cylinders. The difference observed is the indicated horsepower of the cylinder which is out of operation. This method almost invariably gives rather too low a figure for the total losses. We have employed another method on our variable-compression engine, which has a very heavy flywheel and a built-up crankshaft mounted on ball bearings. We remove the crankpin and motor the flywheel up to say 2000 r.p.m.; then we switch off the dynamometer and note the deceleration. The crankpin is then replaced and the engine is run under full load at 2000 r.p.m., switched off and the deceleration noted again. From the difference so observed, the total mechanical losses can be estimated over the whole range of speed from 2000 r.p.m. down to zero, or from full speed to stoppage of the engine.

In an engine such as my variable-compression unit, which has been in constant use for over 2 years, running daily under close observation, the true mechanical efficiency can be determined by various processes of elimination and by all sorts of cross-checks. All of these methods confirm the fact that the true mechanical efficiency agrees extremely closely with that shown by the motoring test previously described.

APPENDIX 2

Piston Experiments

A great many experiments have been carried out on a special piston-testing machine, shown in Figs. 22, 23 and 24, that consists of a water-jacketed cylinder in which a piston is reciprocated by a swinging-field electric dynamometer. The cylinder is heat-insulated from the crankcase, and the jacket is filled with water that is stirred constantly by a small propeller fitted therein. The machine is, in effect, a calorimeter, and the piston friction, wholly apart from all other sources of friction, can be measured in relative terms by the temperature rise of the jacket water. It is very difficult to obtain reliable absolute figures in this way, because the radiation losses play so important a part, but the relative figures so obtained

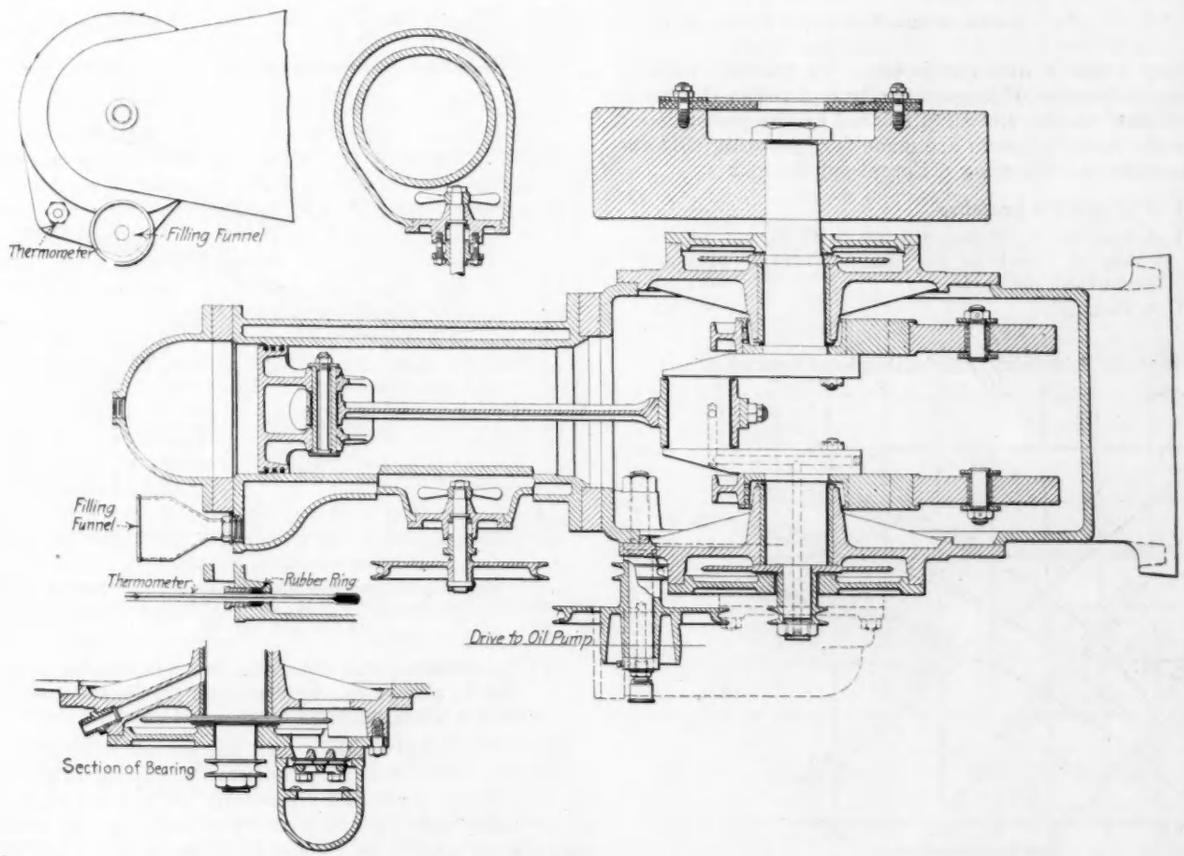


Fig. 23—Longitudinal Elevation, Partly in Section, of the Arrangement Employed by the Author for Testing Piston Friction and Using a Reciprocating Balance

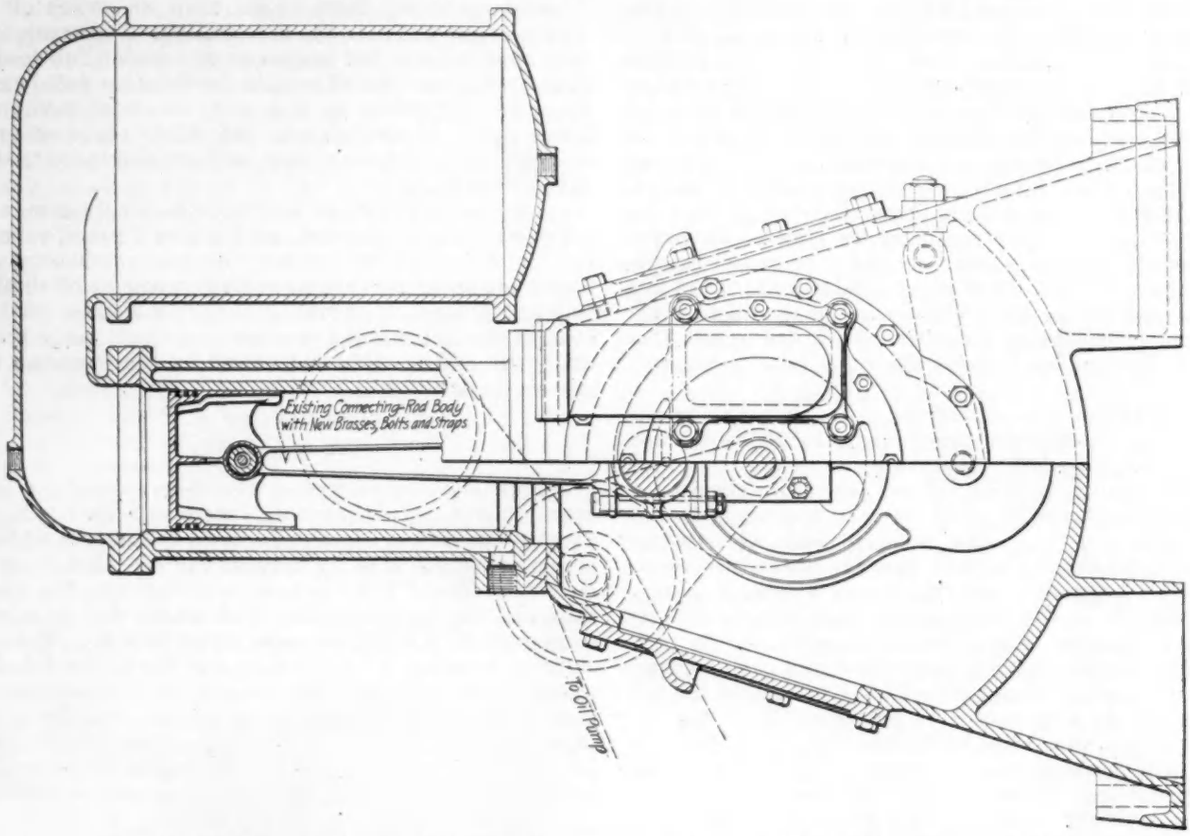


FIG. 24—TRANSVERSE ELEVATION, PARTLY IN SECTION, OF THE AUTHOR'S PISTON-FRICTION TESTING-DEVICE

are very reliable and consistent. To prevent internal cooling, a chamber of large capacity is fitted to the top of the cylinder so that the air displaced by the piston is not changed. Arrangements are made for operating this machine under the following different conditions:

(1) Atmospheric pressure

(2) A continuous air-pressure up to 45 lb. per sq. in., which is equal to the mean average pressure throughout the cycle

(3) A vacuum

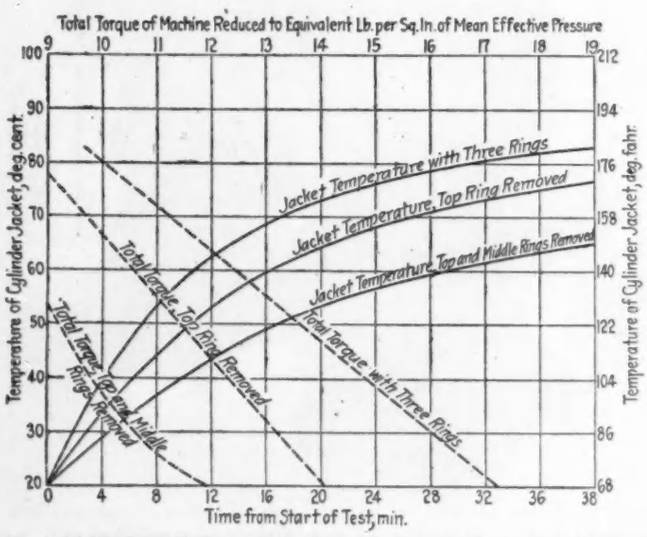

Total Torque of Machine Reduced to Equivalent Lb. per Sq. In. of Mean Effective Pressure
Temperature of Cylinder Jacket, deg. cent
Temperature of Cylinder Jacket, deg. fahr.
Jacket Temperature with Three Rings
Jacket Temperature, Top Ring Removed
Jacket Temperature, Top and Middle Rings Removed
Total Torque with Three Rings
Total Torque, Top Ring Removed
Total Torque, Top and Middle Rings Removed
Time from Start of Test, min.

FIG. 25—TORQUE-TEMPERATURE CURVES OF AN ALUMINUM SLIPPER PISTON OBTAINED AT A SPEED OF 1600 R.P.M. AND NO AIR PRESSURE

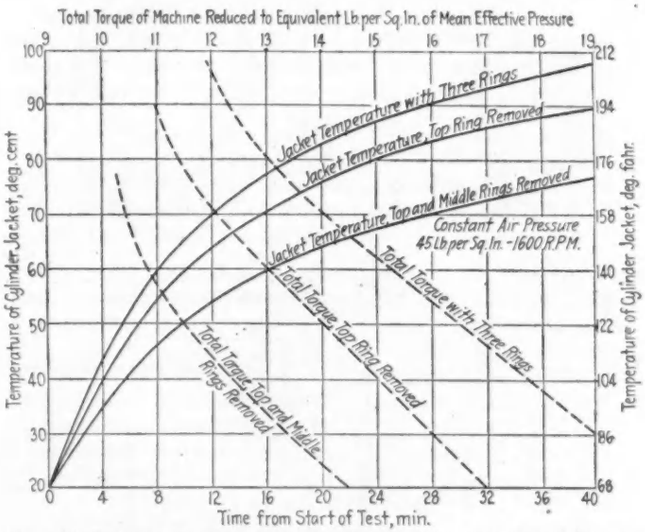

Total Torque of Machine Reduced to Equivalent Lb. per Sq. In. of Mean Effective Pressure
Temperature of Cylinder Jacket, deg. cent
Temperature of Cylinder Jacket, deg. fahr.
Jacket Temperature with Three Rings
Jacket Temperature, Top Ring Removed
Jacket Temperature, Top and Middle Rings Removed
Constant Air Pressure 45 Lb per Sq. In. - 1600 R.P.M.
Total Torque with Three Rings
Total Torque, Top Ring Removed
Total Torque, Top and Middle Rings Removed
Time from Start of Test, min.

FIG. 26—TORQUE-TEMPERATURE CURVES OF AN ALUMINUM SLIPPER PISTON OBTAINED AT 1600 R.P.M. AND A CONSTANT AIR PRESSURE OF 45 LB. PER SQ. IN.

(4) Compressing and expanding to a pressure of about 150 lb. per sq. in., by replacing the large-capacity with a plain cylinder-cover

The rate of gas leakage past the piston is determined by fitting a small and very light automatic air-inlet valve in the cylinder cover and measuring the amount of make-up air taken into the cylinder when compressing and expanding. Figs. 25, 26, 27 and 28 show some typical time-temperature diagrams taken with this machine and also the gross torque of the whole machine as measured by the

dynamometer. The latter should, of course, be read on the top horizontal scale of the diagram, against the temperature. The following deductions can be drawn from various tests on this machine.

(1) Piston friction is affected comparatively little by the fluid pressure on the piston

(2) Piston-rings account for a substantial proportion of the total piston friction

(3) Piston friction is at a minimum when compressing and expanding to such a pressure as to equal the inertia pressure; it is then abnormally low

(4) The amount of oil passed by any one type of piston is dependent only upon the speed and is proportional to it. At any given speed, it is practically unaffected by the pressure in the cylinder from a vacuum of 10 lb. per sq. in. to a positive pressure of 45 lb. per sq. in. This agrees with my experience with actual engines. I have always found the oil consumption to be a direct function of the speed and wholly independent of throttle opening, which latter determines, rather, whether or no the oil passed by the piston is completely burned

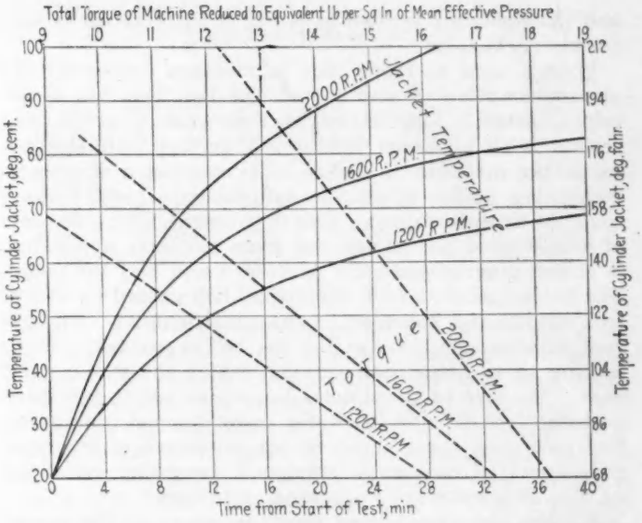

Total Torque of Machine Reduced to Equivalent Lb per Sq In of Mean Effective Pressure
Temperature of Cylinder Jacket, deg cent.
Temperature of Cylinder Jacket, deg fahr
Time from Start of Test, min
2000 R.P.M. Jacket Temperature
1600 R.P.M.
1200 R.P.M.
Torque 2000 R.P.M.
1600 R.P.M.
1200 R.P.M.

FIG. 27—TORQUE-TEMPERATURE CURVES OF AN ALUMINUM SLIPPER PISTON OBTAINED WITH NO AIR PRESSURE AND ALL THREE RINGS IN PLACE

APPENDIX 3.

AIR MEASUREMENT

When investigating the performance of an internal-combustion engine it is essential, of course, to isolate and determine separately the influence of each of the main controlling factors. Above all, we must know both the mechanical and the combustion efficiencies, because it is upon these two factors that the power output and overall efficiency of an engine ultimately depend. We can arrive at the mechanical efficiency very easily and very closely by the motoring test, so long as we are careful to insure that this is carried out as nearly under running conditions as is possible.

The combustion efficiency, which is generally the most important factor of all, is very difficult to determine in the case of multi-cylinder engines. It is generally impossible to arrive at any just conclusion as to the combustion efficiency of a multi-cylinder engine by the usual method of measuring the power output and fuel consumption, on account of the large irregularities in distribution which are bound to occur. Thus, we may, and frequently do, find that one engine with a most efficient form of combustion-chamber and the like actually consumes more fuel per horsepower-hour than another of much less efficient design, simply because the distribution system of the latter is more uniform. At first sight, one might be led to suppose that, since the overall thermal efficiency of the second engine is higher, this therefore is inherently the more efficient engine of the two. However, by comparing the maximum mean effective pressure obtained from the two engines at their most favorable speeds, we shall have a more reliable comparison because, within reasonable limits, we are justified in assuming that however different the valve-timing, area and the like may be, the *maximum* volumetric efficiency of both engines will be about the same at some point in their speed range, and that each, at its most favorable speed, takes into the cylinder very nearly the same proportionate weight of working fluid per cycle. If this assumption were strictly correct, the maximum indicated mean pressure obtained by either engine would give an exact measure of the efficiency of combustion. Such an assumption cannot be confirmed without actual air measurement, although, as between generally similar engines designed to run at somewhat similar speeds, such an assumption probably is very nearly correct, provided always, of course, that the heat supplied to the carbureter and induction system bears the same relation to the weight of fuel in each case.

That the heat-input to the entering charge shall always be supplied in such a manner that it can be measured accurately and in absolute terms, that is, by electric-heater elements, is most desirable for all reliable research work and, unless means are available for measuring the air consumption accurately, it is a first essential. Whatever the variants may be, there is no doubt that, in the case of multi-cylinder engines, the true engine efficiency, as contrasted with the overall efficiency of engine and distribution system together, can be determined far more accurately from the maximum mean effective pressure developed than from any measurements of fuel consumption. Further, if the weight of air taken into the cylinders in a unit time can be measured, we have a means of ascertaining very closely the true thermal efficiency of an engine, no matter how irregular its distribution or how erratic its carbureter-setting may be, because, once we know definitely what use an engine makes of the air supplied to it, we know its true combustion efficiency; it is then incumbent upon the designer of the carbureter

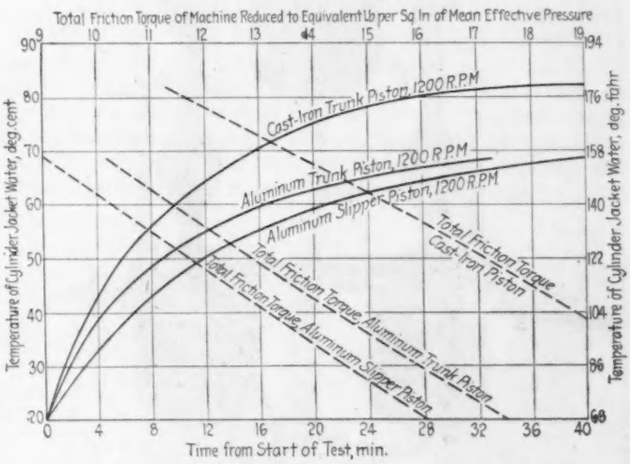

Total Friction Torque of Machine Reduced to Equivalent Lb per Sq In of Mean Effective Pressure
Temperature of Cylinder Jacket Water, deg cent
Temperature of Cylinder Jacket Water, deg fahr
Time from Start of Test, min.
Cast-Iron Trunk Piston, 1200 R.P.M.
Aluminum Trunk Piston, 1200 R.P.M.
Aluminum Slipper Piston, 1200 R.P.M.
Total Friction Torque Cast-Iron Piston
Total Friction Torque Aluminum Trunk Piston
Total Friction Torque Aluminum Slipper Piston

FIG. 28—TORQUE-TEMPERATURE CURVES OF A CAST-IRON TRUNK, AN ALUMINUM TRUNK AND AN ALUMINUM SLIPPER PISTON

and the induction system to see to it that the air is uniformly carbureted.

With a view to being able to measure accurately the air consumption of engines on the test bed, we determined, after a long series of somewhat disappointing results with air-boxes, calibrated orifices and electric-resistance methods, to design and construct a direct air-measuring device about the calibration of which there could be no uncertainty. This instrument, which consists of a calibrated gas-holder and some auxiliary apparatus, is shown diagrammatically in Figs. 4 and 5 of the paper. The holder consists of a sheet-steel bell guided by a central column and balanced in the usual manner. To prevent surging, the lower end of the bell is partially closed, leaving three holes open through which the water must pass. The size of these holes is such as not to interfere with the free flow of the water under normal conditions. For recording the amount of air consumed, a magnetically operated counter is employed, recording every 0.2 cu. ft. This counter is operated as follows:

A roller chain connected near the center of the crown of the bell passes over a sprocket pulley and is kept taut by the balance weight. A fine-pitch ratchet-wheel is mounted on the spindle of the chain-sprocket wheel and driven by a light friction clutch so that it rotates with it as the bell falls but remains stationary while it rises. This ratchet wheel operates a low-tension contact-maker, arranged so that contact is made and broken by each tooth of the ratchet. The contact-maker is connected up to an electromagnetically operated counter placed on the operator's desk, which records the number of contacts made or broken so long as the circuit is maintained. The pitch of the ratchet-wheel is designed so that contact is made and the counter advanced one digit for every 0.2 cu. ft. of air drawn from the holder.

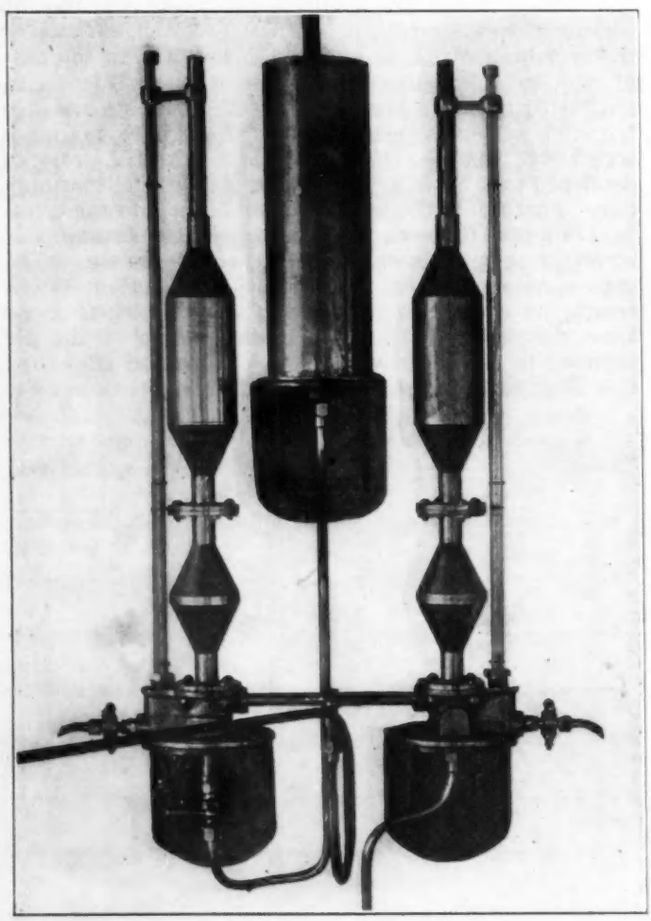


FIG. 29—PHOTOGRAPH OF THE FUEL-MEASURING DEVICE USED

A double-beat valve faced with rubber, also operated magnetically, is fitted to the air intake of the carbureter and arranged so that the supply of air to the engine can be drawn either from the atmosphere or from the holder, depending upon which face the valve is seating. The valve seats on the upper face under normal conditions, and air enters through the lower port from the atmosphere. The valve is snapped over from the upper to the lower seat by bringing the solenoid into operation, and air is then drawn from the holder. A small centrifugal fan driven by an electric motor is employed for charging the holder. This fan delivers air into the holder through an oil seal that forms an automatic non-return valve, and continues to do so until the bell reaches the top of its travel, when the latter automatically opens a switch and so stops the fan. The stopping of the fan permits the oil in the sealing box to flow back into the air inlet, and so prevents any air from being sucked through into the bell in either direction.

For carrying out fuel and air-consumption tests, the operation is as follows so soon as the observer is satisfied that the engine is developing its normal power output and that all temperature and other conditions have settled down to their normal value, he opens the cock and fills the fuel-measuring device from the main tank, as shown in Fig. 29; then he starts the fan to fill the holder. These operations occupy only a few seconds. As soon as the fuel-measuring device and the holder are filled with fuel and air, he closes the fuel cock, throws over the double-beat valve so that the engine then draws its fuel and air from their respective measuring devices, and then watches the rapid fall of fuel in the gage glass until it reaches the first mark. As the level of the fuel falls rapidly past this mark, he closes a switch and starts his stop-watch. The closing of the switch operates the revolution counter on the engine and the air-consumption counter simultaneously. The operator then waits until the level of the fuel falls past the second mark on the gage glass; at this instant, he closes the switch and stops his watch. He then has exact records available; they are (*a*) the number of revolutions during the consumption of a measured quantity of fuel; (*b*) the number of cubic feet of air consumed; and (*c*) the time taken during the consumption of fuel and air, this being required merely to give a true reading of the average engine speed, and as a check on the tachometer.

Repeated experiments made by different observers have indicated that the variation between different observers, due to the relatively small influence of the personal element, does not exceed about 0.50 per cent in practice; different readings by the same observer seldom vary more than 0.25 per cent and the absolute accuracy of measurement of an aggregate of several tests is probably in the neighborhood of 0.10 per cent. Errors due to changes of temperature of the air in the air-holder, because of sunshine, rain and the like, are reduced to the lowest possible minimum by the speed of operation. The holder is filled in about 30 sec. and its contents withdrawn in about 150 sec. on an average; thus, the length of time during which the bulk of the air remains in the holder is reduced to little more than 3 min. from start to finish. Readings are taken of the temperature of the air entering the air-bell, at the double-beat valve and close to the carbureter. Under ordinary atmospheric conditions, the

temperature at these three points seldom varies by more than 1.5 deg. cent. (2.7 deg. fahr.).

Experiments with the air-measuring device coupled to our variable-compression engine have shown that, over a wide range of mixture-strength, the relationship between the indicated mean pressure and the weight of air entering the cylinder is practically constant; at all events, over the range from 5 to 35 per cent rich; and almost any distribution system, however bad, can be relied upon to keep within these limits. In practice, to determine the true combustion efficiency of any engine, it is necessary merely to adjust the carbureter to give a mixture for maximum power, that is, somewhere about 20 per cent rich, and to determine the efficiency from the ratio

$$E = P \div W C$$

where

C = a constant = 1.96 for rich gasoline mixtures
E = the efficiency
P = indicated horsepower
W = pounds of air per hour

TABLE 5—AIR CONSUMPTION

Fuel	Effective Lower Calorific Value, B.t.u. per lb.	Air-Fuel Ratio, at Correct Mixture-Strength	Heat Liberated by 1 Lb. of Air, B.t.u.	Value of Constant, C
Gasoline Samples				
A	19,200	15.05	1,275	1.940
B	19,020	14.70	1,295	1.975
C	19,120	14.80	1,293	1.970
D	18,900	14.60	1,295	1.970
E	19,090	14.90	1,282	1.950
F	19,250	15.00	1,285	1.955
G	18,920	14.70	1,288	1.960
Kerosene	19,100	15.00	1,275	1.940
Hexane	19,390	15.20	1,275	1.940
Heptane	19,420	15.10	1,285	1.955
Benzene	17,460	13.20	1,320	2.010
Toluene	17,660	13.40	1,315	2.000
Cyclohexane	18,940	14.70	1,290	1.965
Heptylene	19,320	14.70	1,320	2.010
Ether	16,830	13.00	1,295	1.975
Ethyl Alcohol,				
99 per cent	11,950	8.95	1,335	2.020
95 per cent	11,125	8.40	1,330	2.015

Table 5 gives the total effective internal energy liberated by 1 lb. of air when fully saturated with various fuels. These values must not be confused with the figures given previously for the internal energy of different fuel-air mixtures, or with the volumetric efficiency figures, both of which include the fuel vapor as well as the air. Although somewhat empirical, the figures given in the third and fourth columns are fairly accurate. From the figures in Table 5 it will be seen that, whatever fuel is used, the energy liberated by 1 lb. of air will always be approximately 1300 B.t.u. The extreme variation over the entire range of representative fuels is from 1275 to 1335 B.t.u., while the variation over the whole range of gasolines is only from 1275 to 1295 B.t.u., depending upon the proportion of *aromatic* present in the fuel.

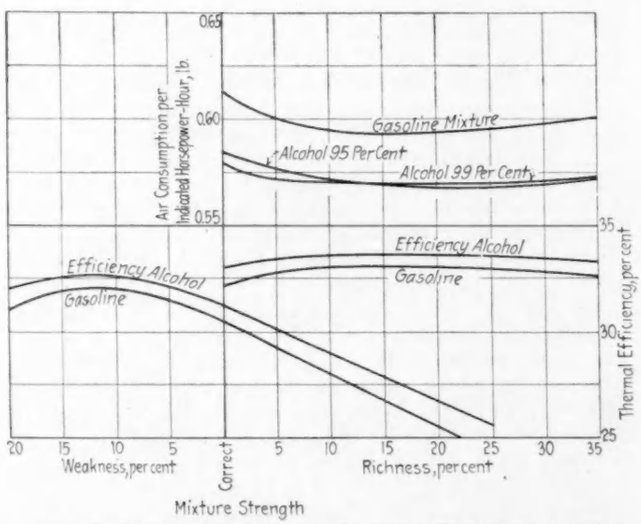


FIG. 30—CURVES SHOWING THE THERMAL EFFICIENCY AND AIR CONSUMPTION FOR VARIOUS MIXTURE-STRENGTHS

TABLE 6—TEST RESULTS ON GASOLINE SAMPLE D WITH A COMPRESSION-RATIO OF 5 TO 1

Mixture Strength, per cent	Air Consumption Per Hour, lb.	Indicated Mean Effective Pressure, lb. per sq. in.	Indicated Horsepower	Air Consumption Per Indicated Horsepower-Hour, lb.	Indicated Thermal Efficiency, per cent
Correct	196.0	132.0	32.0	6.13	32.10
+ 5	196.5	135.0	32.7	6.00	32.80
+ 10	197.0	136.5	33.1	5.95	33.10
+ 15	197.5	137.5	33.3	5.93	33.20
+ 20	198.1	138.0	33.4	5.94	33.15
+ 25	198.8	138.0	33.4	5.96	33.05
+ 30	199.5	137.5	33.3	5.98	32.85
+ 35	200.0	136.5	33.1	6.04	32.70

The following test figures in Tables 6 to 9 and the curves of Fig. 30 are taken from actual tests conducted in the manner already described. Table 6 shows the total air-consumption, the indicated mean pressure, the pounds of air per indicated horsepower and the indicated thermal efficiency, calculated from air consumption, on gasoline, with a compression-ratio of 5 to 1, with a constant heat-input of 0.0433 B.t.u. per revolution and at a constant speed of 1500 r.p.m. Table 6 gives the results obtained experimentally over a range of mixture-strength from that giving complete combustion with no excess of air up to a mixture-strength that has a 35-per cent excess of gasoline.

Table 7 shows the same determination for 99-per cent ethyl alcohol and Table 8 the same for 95-per cent ethyl alcohol. It will be observed from Tables 7 and 8 that the air consumption in pounds per indicated horsepower-hour is a little lower in the case of the alcohols than in the case of gasoline. This is due partly to the fact that, as shown previously, the total heat energy liberated by the consumption of 1 lb. of air is very slightly greater in the case of alcohol and in part also to the fact that, owing to the lower mean cycle temperature, the efficiency obtained on alcohol is always some 2 to 3 per cent higher than on gasoline or benzene, as shown in Fig. 3.

It will be observed that in all cases the air consumption in pounds per indicated horsepower-hour is the same to within very close limits over the whole range of mixture-strengths from 5 to 35 per cent rich. That is, no matter what the mixture-strength may be, so long as there is an excess of fuel present, the thermal efficiency, based on the air consumption or, more strictly speaking, on that portion of the fuel that is burnt, is virtually the same. Incidentally, it is interesting to note that, as the

TABLE 7—TEST RESULTS ON 99-PER CENT ETHYL ALCOHOL WITH A COMPRESSION-RATIO OF 5 TO 1

Mixture Strength, per cent	Air Consumption Per Hour, lb.	Indicated Mean Effective Pressure, lb. per sq. in.	Indicated Horsepower	Air Consumption Per Indicated Horsepower-Hour, lb.	Indicated Thermal Efficiency, per cent
Correct	199.0	141.0	34.20	5.82	32.90
+ 5	199.5	143.0	34.60	5.77	33.30
+ 10	200.0	144.5	35.00	5.72	33.65
+ 15	201.0	145.5	35.25	5.70	33.70
+ 20	202.0	146.5	35.50	5.69	33.75
+ 25	203.0	147.0	35.60	5.70	33.70
+ 30	204.0	147.3	35.70	5.72	33 65
+ 35	205.0	147.6	35.80	5.73	33.60

TABLE 8—TEST RESULTS ON 95-PER CENT ETHYL ALCOHOL WITH A COMPRESSION-RATIO OF 5 TO 1

Mixture Strength, per cent	Air Consumption Per Hour, lb.	Indicated Mean Effective Pressure, lb. per sq. in.	Indicated Horsepower	Air Consumption Per Indicated Horsepower-Hour, lb.	Indicated Thermal Efficiency, per cent
Correct	203.0	145.0	35.10	5.79	33.10
+ 5	203.5	146.7	35.50	5.72	33.60
+ 10	204.0	148.0	35.80	5.71	33.65
+ 15	205.0	149.0	36.00	5.70	33.70
+ 20	206.0	149.5	36.15	5.70	33.70
+ 25	207.0	150.0	36.25	5.70	33.70
+ 30	208.0	150.3	36.30	5.71	33.65
+ 35	209.0	150.5	36.35	5.74	33.40

mixture-strength is enriched beyond the point of complete combustion, the thermal efficiency, based on that portion of the fuel which is utilized, rises at first and then remains nearly constant over a wide range; but, in every case, it actually reaches a maximum at about 20 per cent rich. The curves in Fig. 30 show the thermal efficiency as ascertained in this manner over a range of mixture-strengths from correct to 35 per cent rich. The efficiency on the weak side, as determined from the fuel consumption alone, is shown in dotted lines. That the efficiency as reckoned by the air consumption should be so uniform over a wide range of mixture-strengths on the rich side, as it has been found to be, is perhaps somewhat of a coincidence in view of the wide variation in the nature of the chemical reactions, the change of specific volume and the like.

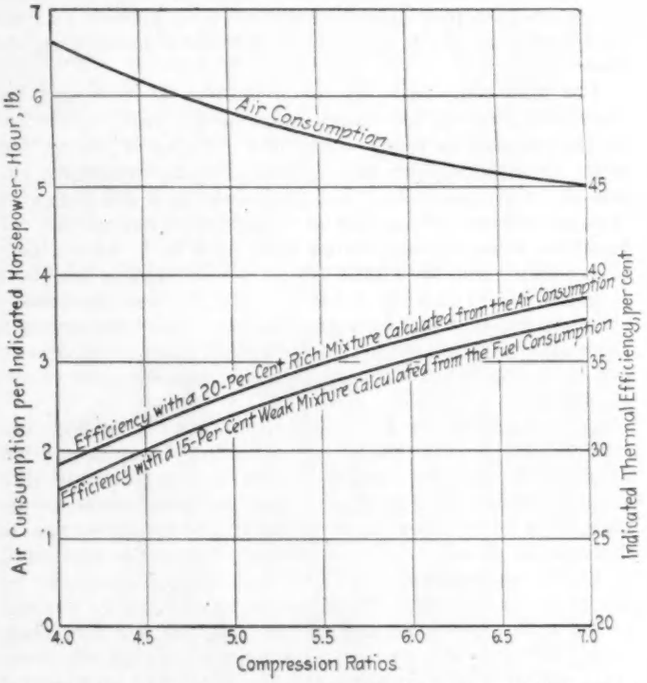


FIG. 31—THERMAL EFFICIENCY AND AIR CONSUMPTION CURVES FOR VARIOUS COMPRESSION RATIOS

TABLE 9—TEST RESULTS WITH FUEL BENZOL, ABOUT 20-PER CENT RICH; VARYING COMPRESSION-RATIO

Compression-Ratio	Air Consumption Per Hour, lb.	Indicated Mean Effective Pressure, lb. per sq. in.	Indicated Horsepower	Air Consumption Per Indicated Horsepower-Hour, lb.	Indicated Thermal Efficiency, as Found by Air Measurement, per cent	Indicated Thermal Efficiency, as Found by Fuel Measurement, 15-Per Cent Weak, per cent
4 to 1	200.5	125.0	30.3	6.62	29.2	27.7
5 to 1	194.0	136.5	33.1	5.82	33.2	32.0
6 to 1	188.0	145.0	35.2	5.34	36.2	35.0
7 to 1	184.0	152.0	36.8	5.00	38.5	37.3

Table 9 and Fig. 31 show the efficiency as determined from the air consumption when running on benzol with a mixture-strength varying anywhere from 10 to 30 per cent rich, and at compression-ratios ranging from 4 to 1 to 7 to 1, while the lowest line shows the maximum thermal efficiency as ascertained from the fuel consumption with the mixture adjusted in each case to give the highest economy; namely, about 15 per cent weak. The efficiency as calculated from the air consumption will always be slightly higher than that determined by the fuel consumption, the slight difference being due in part to the small proportion of fuel that is lost in carburetion or otherwise escapes combustion by adhering to the cold cylinder-walls. As a result of these investigations, I am convinced that, for tests on multi-cylinder engines in which it is impossible to insure that each cylinder receives a mixture of exactly the same strength, the air-measurement test gives a very much closer approximation to the true thermal efficiency of such an engine than can possibly be arrived at from the fuel consumption; and that, where means are not available for measuring the air consumption accurately, the maximum indicated mean effective pressure, suitably corrected for the compression-ratio, gives a fairly true comparison at all events in the case of engines of generally similar type and size.

It may be argued that to reckon the thermal efficiency of an engine by its air consumption is of academic interest only, and that it is the efficiency on fuel consumption alone that is of any practical or commercial value. Such an argument is, however, fallacious; if it can be shown that an engine consumes its air efficiently, then it is essentially an efficient engine, and to render it efficient also on fuel is purely a matter of carburetion and distribution.

APPENDIX 4

INTERNAL ENERGY OF THE WORKING FLUID

Some years ago the Gaseous Explosions Committee of the British Association prepared a curve showing the total internal energy of the working fluid comprising the products of combustion of an air-gas mixture over a wide range of temperature. This curve was constructed after a very careful analysis of all the reliable investigations available at the time. It would appear, however, in the light of more recent knowledge, that the change in apparent specific heat at the higher temperatures has been somewhat over-rated; also it is applicable only to a mixture of coal gas and air, the characteristics of which differ considerably from those of an air-gasoline mixture.

From the results of Tizard and Pye's investigations, it becomes possible to construct a similar curve for a gasoline-air or benzene-air mixture. Such a curve is shown in Fig. 32 and is probably about as accurate as the present state of knowledge will permit. From such a curve, and by making certain assumptions, it is possible to deduce the temperature of the working fluid at almost any point in the cycle. Fig. 32 shows a graphic construction by which, once the heat of combustion of the fuel and the thermal efficiency are known, the range of temperature throughout the expansion-stroke can be determined with a fair degree of accuracy. In the construction of such a graph and to keep it reasonably applicable to a wide range of conditions, it is necessary to compromise to some extent and to take mean values.

Thus, the energy curve shown applies strictly only to

a benzene-air complete-combustion mixture at a compression-ratio of 5 to 1. However, the effect of a change in the compression-ratio is negligible within the limits used on the constant-volume cycle; as is the effect, also, of replacing benzene by any other hydrocarbon fuel. If used for a fuel such as alcohol or ether, however, the error becomes appreciable, owing to the different specific heat of the products of combustion. For the same reason, the curve does not apply to mixtures which are either weaker or richer than the chemically correct mixture, although here again the divergence is very small within the range available with a homogeneous mixture.

In Fig. 32, the internal energy curve is plotted in terms of British thermal units per standard cubic inch on a vertical scale, against the temperature on a horizontal scale. The other full-line curve shows the energy present as heat, so that the difference between the two curves shows the chemical energy stored in the products of dissociation. Zero energy is taken at 100 deg. cent. (212 deg. fahr.) as being an average temperature at the beginning of compression. Variations in this temperature will have but little influence. The explanation given below of the use of the diagram is supplemented by an example worked out for the following data, the construction lines of the example being shown dotted in Fig. 32.

Compression-ratio, R,	= 5 to 1
Energy content	= 46.2 B.t.u. per cu. in.
Heat loss during combustion	= 6 per cent
Heat loss during expansion	= 6 per cent

There are three factors in an actual engine which modify the temperature attained by the combustion of a mixture of any given energy content. They are the

(1) Heat put into the mixture by compression
(2) Loss due to cooling by the walls of the combustion-chamber during combustion
(3) Effective weakening of the mixture due to dilution with the residual exhaust products

Factor (1) is allowed for by laying off the heats of compression for various ratios by the marks "R = 5," etc., on the line $P\ P_1$ near the bottom of the diagram. The energy content is then marked off above this on the vertical line $O_1\ Y_1$ representing the 100 deg. cent. (212 deg. fahr.) starting-point. In the example, the 46.2 B.t.u. energy content is laid off above the 3.6 B.t.u. of compression, making a total of 49.8 B.t.u., this being the gross energy content from which the losses due to factors (2) and (3) must be deducted. This is done in the following manner:

On the horizontal scale C is marked the effective energy loss due to dilution with residual exhaust, assumed to be at 1000 deg. cent. (1832 deg. fahr.). Scale E shows the percentage loss due to cooling during combustion. This is laid off at any figure which previous experience shows as probable for the type of combustion-chamber in question; this is 6 per cent in the example. A line is then drawn between these two points, and the point of intersection of this line with the scale D gives the total percentage loss due to these two causes; this is 11.5 per cent in the example.

To transfer this to the diagram, a line is dropped vertically from the above intersection point. Another line is drawn from the point on the line $O_1\ Y_1$ giving the gross British thermal units per cubic inch to the suitable compression point on the line $P\ P_1$, and representing 100 per cent on scale D; this is 49.8 B.t.u. per cu. in. in the example. From the intersection of the above two lines, a horizontal line is run to the energy scale on one side.

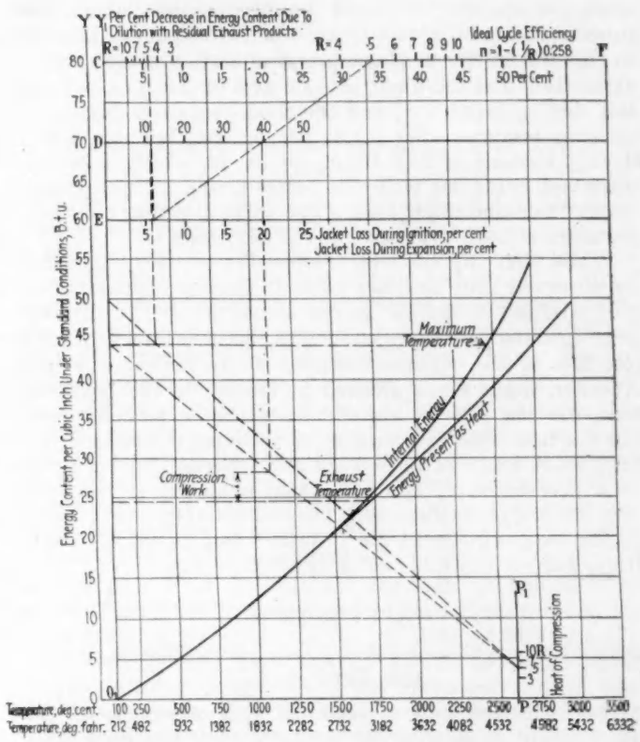

Y Y, Per Cent Decrease in Energy Content Due To Dilution with Residual Exhaust Products
R=10 7 5 4 3
R=4 5 6 7 8 9 10
Ideal Cycle Efficiency n=1-(1/R)0.258
F
C
D
E
50 Per Cent
25 Jacket Loss During Ignition, per cent
Jacket Loss During Expansion, per cent
Maximum Temperature
Internal Energy
Energy Present as Heat
Compression Work
Exhaust Temperature
Energy Content per Cubic Inch Under Standard Conditions, B.t.u.
P1
10R
5
3
Heat of Compression
O1
Temperature, deg. cent. 100 250 500 750 1000 1250 1500 1750 2000 2250 2500 P 2750 3000 3500
Temperature, deg. fahr. 212 482 932 1382 1832 2282 2732 3182 3632 4082 4532 4982 5432 6332

FIG. 32—INTERNAL ENERGY DIAGRAM FOR THE WORKING FLUID OF AN INTERNAL-COMBUSTION ENGINE RUNNING ON VOLATILE HYDROCARBON FUELS

and the energy curve on the other. The point on the energy scale shows the net energy available for expansion; it is 44.5 B.t.u. per cu. in. in the example. From the energy-temperature curve, the actual flame temperature can be read off; this is 2475 deg. cent. (4487 deg. fahr.) in the example.

The drop in temperature during the expansion-stroke depends on the two factors of (a), external work done and (b) heat loss to the walls. The net power output is given as a percentage of the heat content of the mixture on scale F, the formula used being $n = 1 - (1/R)^{0.258}$. This covers all dissociation and similar effects, but not wall losses. The wall loss during expansion is laid off on scale E. A line drawn between these points gives their sum on scale D as before. A perpendicular from this point is dropped to meet a line from the net-energy point on $O_1\ Y_1$ to the suitable-compression point on the line PP_1. As the gross work done during expansion is the sum of the net work mentioned above and the compression work, this latter amount, which is 3.6 B.t.u. in the example, must be laid off below the above intersection point, to find the energy content at the end of expansion, which is 24.5 B.t.u. in the example. The corresponding final temperature, 1675 deg. cent. (3047 deg. fahr.) in the example, can then be read off from the energy curve.

Taking an actual example from the variable-compression-engine data, with a correct mixture of an energy content of 46.2 B.t.u. per cu. in. and a compression-ratio of 5 to 1, the actual maximum flame-temperature, as obtained from the above diagram in Fig. 32, allowing for the additional heat of compression, the wall loss during combustion and the dilution by residual exhaust products, will be 2475 deg. cent. (4487 deg. fahr.), corresponding to an energy content of 44.5 B.t.u. per standard cu. in. At a ratio of 5 to 1, the observed indicated thermal efficiency is 31 per cent; of this, 5 per cent is due to the

change in specific volume of the mixture, so that the heat drop is 46.2 x 31 x 100/105 = 13.6 B.t.u. per cu. in. Add to this the 3.6 B.t.u. of compression work restored during expansion, and the 6 per cent of 46.2 or 2.8 B.t.u. of wall loss during expansion, and the total heat-drop during expansion becomes 20.0 B.t.u. per cu. in., leaving a final energy content of 24.5 B.t.u. per cu. in. which, it will be observed, coincides with the figure found in the example under the same conditions. The corresponding final temperature is 1675 deg. cent. (3047 deg. fahr.).

While affecting the final temperature directly, it should be observed that the loss of heat during expansion has only a slight influence on the actual efficiency; this has been ignored in the construction, because much of it is lost late in the expansion-stroke where its value is less. Another slight error allowed to remain in the construction, for the sake of simplicity, is that a percentage of the *net* heat available during combustion is deducted for the jacket loss during expansion; whereas this is given as a proportion of the *total* heat available in the fuel. The error due to this cause is however very small, being in the case considered 2.8 [(46.2 — 44.5) ÷ 44.5] = 0.11 B.t.u. per cu. in., and can be ignored safely.

APPENDIX 5

Influence of Compression-Ratio Upon Power Output and Efficiency

The influence, both theoretical and observed, of the compression-ratio upon thermal efficiency has been dealt with in the main portion of the paper and Table 3 gives both Tizard and Pye's deductions and the observed figures over a range from 4.0 to 1 up to 7.5 to 1. The outstanding feature of all of the experiments has been the exceedingly close agreement between the theoretical and the observed thermal efficiencies throughout practically the whole range with the exception, perhaps, of the lower compressions where the observed efficiency is somewhat lower than would be expected. As regards power output, however, there is very marked divergence between the observed and the theoretical results, because the indicated mean effective pressure does not rise with increase of compression at anything approaching the rate at which one might expect. It is obvious that, if all mechanical and temperature conditions remain constant, the increase in both efficiency and power output with an increase of compression must also be the same, unless the volumetric efficiency undergoes some change. That the mechanical conditions remain unaltered is certain, for the change in compression is effected by raising or lowering the whole cylinder together with its overhead camshaft, carbureter, induction pipe and ignition gear; while, as to the temperature conditions, the only respect in which they can alter appreciably is that the exhaust-valves are somewhat cooler at the higher compression-ratios. In fact, as shown in Appendix 8, the total heat-flow to the cylinder walls is, if anything, slightly lower at the higher compression-ratios. Therefore, such changes of temperature as can occur will tend to increase the weight of charge retained in the cylinder, if, indeed, they are of sufficient magnitude to have any appreciable influence. Despite this, however, the volumetric efficiency has been found to fall rapidly with increase of compression. The reason for this is still very obscure. If it occurred at one speed only it might be attributed to resonance or other effects in the pipe work or, as W. S. James suggested to me, to the greater depression in the cylinder at the moment when the inlet-valve first opens. Observations at widely varying speeds have shown, however, that the change in volumetric efficiency is, in fact, substantially the same at all speeds; and recent experiments, carried out at the Royal Aircraft Establishment on another engine in which the compression could be varied by inserting packing-pieces under the cylinder, have given similar results. Further, the general experience with aircraft engines has shown that, while the use of a high compression-ratio greatly increases the efficiency, the gain in power output is much less marked.

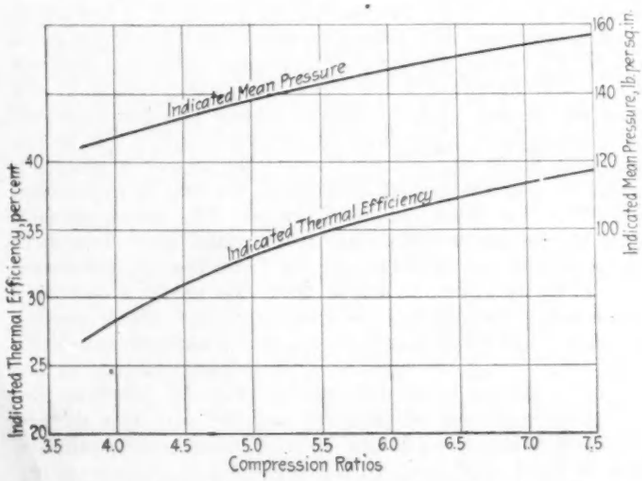


Fig. 33—Indicated Mean Effective Pressure and Thermal Efficiency Curves for 95-Per Cent Ethyl Alcohol at Varying Compression-Ratios

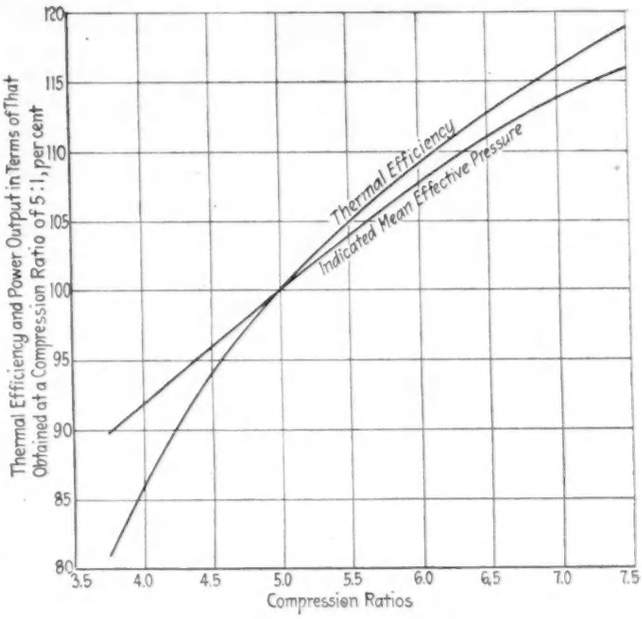


Fig. 34—Correction Curves for Indicated Mean Effective Pressure and Thermal Efficiency

Fig. 33 shows the variation both in thermal efficiency and in mean effective pressure as observed over a range of compression from 3.75 to 1 up to 7.5 to 1. The fuel used in this instance was 99-per cent ethyl alcohol, this being chosen as one of the few which could be used safely at the highest compression-ratio. As explained previously, the efficiency and power output on this fuel are both somewhat higher than on gasoline or benzol. Within their safe limits, however, all of the other fuels tested have shown precisely the same characteristic variation. Fig. 34 shows the correction curves in terms of percent-

age, which can be deduced from the observed results as shown in Fig. 33.

Further tests, with the air-measuring device in use, have shown that as the compression-ratio is raised the weight of air per cycle is reduced to an extent that accounts almost exactly for the discrepancy. The precise cause of this change in volumetric efficiency still remains somewhat of a mystery. It is possible to produce several plausible arguments to account for an increase in volumetric efficiency with increase of compression, but to account for a decrease is much more difficult.

APPENDIX 6

Influence of Cylinder Size on Performance

The comparative figures given in Table 10 may be of some interest. They are taken from three single-cylinder experimental engines, all of them being similar in that

(1) They had valves fitted directly in the cylinder-heads

(2) They had precisely the same valve-opening diagram

(3) The speed chosen for comparative purposes in all cases is that at which the mean gas-velocity through the inlet-valves is 130 ft. per sec., and through the exhaust 150 ft. per sec.

(4) All three engines were fitted with aluminum slipper-type pistons of similar design

(5) The compression-ratio in all cases was 4.8 to 1, and the fuel used was American aviation gasoline

TABLE 10—COMPARISON BETWEEN SINGLE-CYLINDER EXPERIMENTAL ENGINES

	Engine A	Engine B	Engine C
Bore, in.	3.25	4.50	8.00
Stroke, in.	4.00	8.00	11.00
Brake Horsepower	8.48	35.4	118.0
Speed, r.p.m.	1,750	1,750	1,250
Piston Speed, ft. per min.	1,166	2,333	2,290
Mechanical Efficiency, per cent	83	86	88
Brake Mean Effective Pressure, lb. per sq. in.	114	126	135
Indicated Mean Effective Pressure, lb. per sq. in.	137.5	146.5	153.0
Fuel Consumption, lb. per b.hp-hr.	0.543	0.506	0.474
Fuel Consumption, lb. per i.hp-hr.	0.451	0.435	0.416
Brake Thermal Efficiency, per cent	24.6	26.4	28.2
Indicated Thermal Efficiency, per cent	29.6	30.8	32.0
Relative Efficiency, per cent	63.5	66.1	68.7

From these results it is possible to arrive at an approximate correction-figure for cylinder capacity somewhat as shown in Fig. 35. This must not be taken too seriously, for to generalize from the results of only three engines not entirely similar would be very unsafe, even though much other corroborative evidence has been found in support of the relationship shown in this figure.

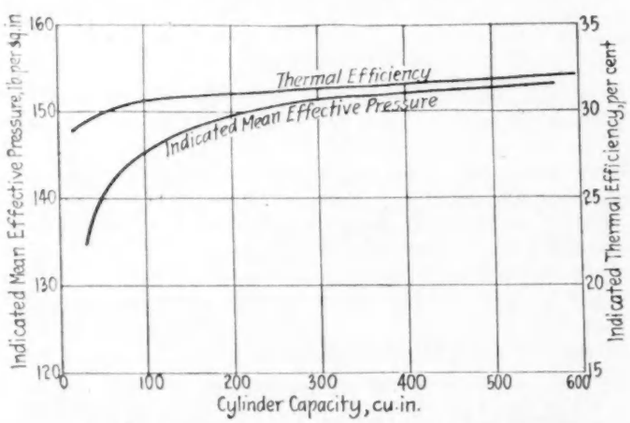


FIG. 35—CORRECTION CURVES FOR CYLINDER CAPACITY

APPENDIX 7

Influence of Cylinder Temperature on Power Output

Under full-load conditions, the indicated horsepower rises as the temperature of the cylinder is reduced, due to the increase in the volumetric efficiency. In the case of our variable-compression engine, which is a fairly normal example, direct measurements of volumetric efficiency have shown that the weight of charge falls 0.5 per cent for every 10-deg. cent. (18-deg. fahr.) rise of mean water temperature. It may be deduced from this that the rise in temperature of the air entering the cylinder in this particular engine amounts to approximately one-sixth of the temperature-difference between the air and the circulating water over the range from 30 to 90 deg. cent. (86 to 194 deg. fahr.). Whether the brake horsepower increases or decreases with the cylinder-jacket temperature depends almost solely upon the piston design. With the ordinary trunk piston having large areas of surface in contact with the cylinder walls, the influence of the viscosity of the oil upon piston friction is such that, despite the decrease in the indicated horsepower with hot jacket-water, the brake horsepower rises or, in other words, with a cold cylinder barrel, the increased friction more than counterbalances the increase in indicated power.

With a very light slipper piston, such as is fitted to our variable-compression engine, the brake horsepower rises very slightly as the temperature of the jacket water is reduced. With crosshead pistons, such as were used in the tank engines, the brake horsepower almost invariably rose by about 2 per cent when the mean jacket temperature was reduced from 80 to 30 deg. cent. (176 to 86 deg. fahr.), or very nearly in proportion to the increase in indicated power over the same temperature range.

APPENDIX 8

Distribution of Heat in a High-Speed Engine

The following test results show the observed distribution of heat in our variable-compression engine under various conditions of operation. Since they were taken in circumstances that make for a very high degree of accuracy, and under a fairly wide range of conditions, they are perhaps of some interest. With the exception of the tests on hydrogen, when the power was controlled by reducing the fuel supply and therefore the flame temperature, there is nothing novel. In all cases, except with hydrogen, the tests were made at a mixture strength ranging from 5 to 10 per cent weak. The tests were carried out in three groups. Group A comprised tests at a constant compression-ratio and constant fuel-air ratio, but with varying speed, on two fuels; also, one series of tests at a higher compression ratio. Group B included tests at constant compression-ratio, constant fuel-air ratio and constant speed, but with the mean effective pressure varied by throttling, on two fuels. Group C was composed of tests at constant compression-ratio and constant speed, but with the mean effective pressure varied by varying the fuel-air ratio as in a Diesel engine; hydrogen gas being used as fuel in this instance. In all cases the following precautions were observed:

(1) The circulating water was maintained at a constant temperature of 60 deg. cent., plus or minus

2 deg. cent. (140 deg. fahr., plus or minus 3.6 deg. fahr.)

(2) The heat-input to the carbureter was adjusted to bear a constant proportion to the weight of fuel consumed, except in the case of hydrogen when the carbureter was unheated

(3) In Groups A and B, the fuel-air ratio in all cases was such as to give approximately 10 per cent excess of air over that required for complete combustion of the fuel, the air consumption being measured and adjusted in each case

(4) No readings were recorded until all temperature conditions had been stabilized for a considerable period after each change of state

The amount of heat dissipated, to the exhaust, by radiation and the like, is arrived at by difference in each case.

In all cases the indicated thermal efficiency can be taken as accurate to within about 0.5 per cent, and the heat to cooling water to within 1 per cent. Also, in all cases the heat produced by piston friction and that lost by radiation balanced at approximately 1500 r.p.m. The water temperature at which the readings were taken was that which the cylinder attains when motored continuously at 1500 r.p.m.; that is, 45 or 60 deg. cent. (113 or 140 deg. fahr.) above atmospheric temperature.

TABLE 11—TESTS SHOWING HEAT DISTRIBUTION IN A HIGH-SPEED INTERNAL-COMBUSTION ENGINE

Group A—Compression-Ratio, 3.8 to 1; Fuel, 95-Per Cent Ethyl Alcohol				
Engine Speed, r.p.m.	975	1,300	1,500	1,700
Piston Speed, ft. per min.	1,300	1,733	2,000	2,266
Heat to Indicated Horsepower, per cent	26.9	27.0	26.9	27.0
Heat to Cooling Water, per cent	25.1	24.7	24.4	24.2
Heat to Exhaust, Radiation, etc., per cent	48.0	48.3	48.7	48.8
Total Heat, per cent	100.0	100.0	100.0	100.0
Group A—Compression-Ratio, 3.8 to 1; Fuel, Grade A Gasoline				
Engine Speed, r.p.m.	975	....	1,500	1,200
Piston Speed, ft. per min.	1,300	....	2,000	2,266
Heat to Indicated Horsepower, per cent	25.9	....	26.1	26.1
Heat to Cooling Water, per cent	30.4	....	28.0	27.0
Heat to Exhaust, Radiation, etc., per cent	43.7	....	45.9	46.9
Total Heat, per cent	100.0		100.0	100.0
Group A—Compression-Ratio, 7 to 1; Fuel, 95-Per Cent Ethyl Alcohol				
Engine Speed, r.p.m.	975	1,300	1,500	....
Piston Speed, ft. per min.	1,300	1,733	2,000	....
Heat to Indicated Horsepower, per cent	37.6	38.1	38.3	....
Heat to Cooling Water, per cent	25.4	24.3	23.9	....
Heat to Exhaust, Radiation, etc., per cent	37.0	37.6	37.8	....
Total Heat, per cent	100.0	100.0	100.0	
Group B—Throttling at Constant Fuel-Air Ratio, Compression-Ratio, 5.45 to 1; Engine Speed, 1500 r.p.m.; Piston Speed, 2000 ft. per min.; Fuel, 95-Per Cent Ethyl Alcohol				
Percentage of Maximum Indicated Horsepower	100.0	80.0	60.0	40.0
Heat to Indicated Horsepower, per cent	34.8	35.0	35.0	34.8
Heat to Cooling Water, per cent	24.1	26.0	29.2	33.0
Heat to Exhaust, Radiation, etc., per cent	41.1	39.0	35.8	32.2
Total Heat, per cent	100.0	100.0	100.0	100.0
Group B—Compression-Ratio, 5.45 to 1; Engine Speed, 1500 r.p.m.; Piston Speed, 2000 ft. per min.; Fuel, Grade A Gasoline				
Percentage of Maximum Indicated Horsepower	100.0	80.0	60.0	40.0
Heat to Indicated Horsepower, per cent	33.5	34.0	34.1	33.5
Heat to Cooling Water, per cent	26.5	28.2	31.8	35.5
Heat to Exhaust, Radiation, etc., per cent	40.0	37.8	34.1	31.0
Total Heat, per cent	100.0	100.0	100.0	100.0
Group C—Mean Effective Pressure Varied by Varying Fuel-Air Ratio from 15 Per Cent Excess of Air Upward; Compression-Ratio, 5.45 to 1; Engine Speed, 1500 r.p.m.; Piston Speed, 2000 ft. per min.; Fuel, Hydrogen				
Percentage of Maximum Indicated Horsepower	100.0	80.0	60.0	40.0
Heat to Indicated Horsepower, per cent	33.3	35.6	38.2	40.0
Heat to Cooling Water, per cent	23.6	24.9	25.3	28.6
Heat to Exhaust, Radiation, etc., per cent	43.1	39.5	36.5	31.4
Total Heat, per cent	100.0	100.0	100.0	100.0

APPENDIX 9

Efficiency Under Reduced Loads

Tests made recently on our single-cylinder research-engine have shown that, as the load is reduced by throttling and with the ignition set to give the best results on full load, the fuel consumption per indicated horsepower-hour increases slightly as the load is reduced, but that if, at each throttle-opening, the ignition timing is advanced as the load is reduced, the fuel consumption per indicated horsepower-hour remains substantially constant throughout the range from 30 to 100 per cent of full-load torque.

The same procedure was adopted in all these tests, and a very large number have been made on different fuels; that is, at each throttle-opening the whole range of mixture strength was explored. The circulating water was maintained throughout at the same temperature. The heat-input to the carbureter was maintained proportional to the load; that is, at full load the heat-input was at the rate of 65 B.t.u. per min., at one-half load 32.5 B.t.u. per min., and so on. Also, at each throttle position the mechanical losses were measured at intervals by motoring. This was done by switching off the ignition and changing over the armature circuit of the dynamometer simultaneously, the combined operation taking less than 1 sec., and being effected without any appreciable change in speed.

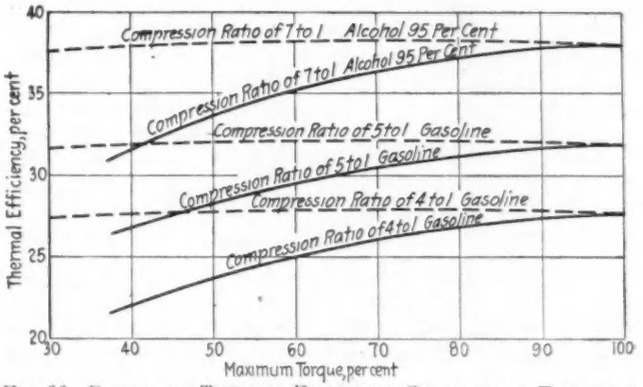


FIG. 36—POWER AND THERMAL EFFICIENCY CURVES FOR A THROTTLED ENGINE USING GASOLINE AND 95 PER CENT ETHYL ALCOHOL AT VARIOUS COMPRESSION-RATIOS

In Fig. 36, full-line curves show the results obtained with fixed ignition and the dotted curves give results with the ignition adjusted for each change in load, in both cases, at three different compression-ratios, on gasoline and alcohol. The former condition, with fixed ignition, is probably the only practical one, but the substantial gain in efficiency effected by advancing the spark when operating on light loads is very noticeable. There is, of course, nothing novel in such experiments except that they were carried out under conditions that insured an unusual degree of accuracy.

THE DISCUSSION

CHAIRMAN H. M. CRANE:—I hope that the members have some realization of the debt we owe Mr. Ricardo for

this paper. Many of us have prepared papers of our own and, even when we have the facts available, the mere assemblage of them into a synopsis of this kind requires a great effort and much time, especially when it is done with the care that has been used in this instance. The problem of distribution is brought out in this paper. Each result of Mr. Ricardo's test indicates the desirability of maintaining a fairly close ratio of gas to air to obtain the best results. It is very evident that this cannot be done in a multi-cylinder engine without securing the most perfect possible distribution of the fuel mixture.

C. P. GRIMES:—I appreciate especially Mr. Ricardo's discussion of fuel conditions on the cylinder wall and turbulence. I made a long series of tests upon some cylinders in which I could control and measure these temperatures very accurately. My ideal was to obtain more power and economy from a given bore and stroke. The economy increased very rapidly with the temperature, up to a 550-deg. fahr. temperature of the combustion-chamber wall. This, I think, bears out very clearly Mr. Ricardo's explanation of the reason for desiring turbulence, to take the fuel from the cool cylinder walls and mix it with the other portions of the charge. I feel certain that the fuel economy I obtained came from heating these walls hot enough to vaporize the fuel from them with what little turbulence existed.

In 1912 a car ran to California and back and employed kerosene as fuel. The products of combustion were used in the carbureter instead of utilizing moisture as had been done previously. I have found tractor builders in favor of such usage but they experience the same result expressed by Mr. Ricardo, in that only about 80 per cent of the power is realized. Maximum economy usually is assured by 15 per cent of excess air. I suppose this is for the purpose of supplying excess oxygen to those few fuel particles that do not utilize it readily. The introduction of exhaust gases reduces the volume of the active charge. I should think this would cancel the benefit derived from the 15 per cent of excess air on account of the increased dilution. I have been able to get economy and power by decreasing the compression and increasing the cylinder-head temperature. These results are at variance with some of Mr. Ricardo's remarks; however, they cover months of experiment and seem to be trustworthy.

I agree with Mr. Ricardo on the sensitiveness of pressure. We ran an engine for 1 hr. at 2250 r.p.m. with varying throttle positions. Toward the last we had a yoke suction of 5.03 in. of mercury, with the throttle about full-open. At the end of 20 min. of running at that position we opened the throttle wide. The cylinder-head temperature had been 554 deg. fahr. but, when we moved the throttle the very small amount required to make it wide-open, the yoke suction was reduced about 0.03 in. of mercury, the cylinder-head temperature increased to 570 and then to 597 deg. fahr. and preignition started.

R. E. WILSON:—Mr. Ricardo has brought out one very important point in emphasizing the fact that pressure is a more important item than temperature in causing detonation. Others have brought out the same point, but Mr. Ricardo has suggested that the chemist claims that pressure could not influence the rate of a chemical reaction principally. As a chemist, I emphatically disagree with that. It is a well-established law, both from a purely theoretical standpoint and from many practical experiments, that although pressure has no effect on the rate of a reaction in the liquid state, where the molecules cannot be brought much more closely together, the increase of pressure in gases does greatly increase the rate of a reaction by bringing the molecules more closely together. The law has been worked out very carefully and it has been proved that the rate of reaction is proportional to the product of the partial pressures of all the molecules reacting, whatever the initial or slowest step in the reaction is. In a reaction like this we have a great many molecules of oxygen, and a single molecule of fuel. However, the *slow* step in the reaction would probably be to get the first molecule or two of oxygen in. Unfortunately, we do not know what that slow reaction is but, if we assume that two molecules of oxygen and one molecule of fuel must combine, then the rate of reaction would increase approximately as the *cube* of the pressure. In other words, Mr. Ricardo has mentioned that increasing from 4 to 6 atmospheres would mean an increase of 50 per cent in the pressure, but an increase of about 340 per cent in the rate of reaction from a theoretical standpoint, which is a much larger increase than one would expect from temperature. If we add to this the increase we get due to the higher temperature only, and also the increase due to having a smaller quantity of exhaust gases present, both of which are real factors, I think we can explain, almost on a quantitative basis, the speeding up of the rate of flame propagation at the higher pressures. But the pressure itself is probably the most important single factor of those three, although the other two enter also.

THOMAS MIDGLEY, JR.:—The original report of the du Pont de Nemours Co. presented the results of some work that was done in photographing the velocity of flame travel and the influence of pressure. The conclusion it reaches is as follows:

> The investigation of the effect of increased initial pressure proved most unsatisfactory, as this work was attempted before the bomb was made gas-tight through sealing the window by gaskets. The leakage made it impossible to control the pressures accurately and, as a result, good checks could not be obtained. The photographs obtained of the 10 and 15-per cent acetylene-air mixtures, respectively, under initial pressures of 7½, 15 and 30 lb. gage, seem to show approximately a 20-per cent increase in the rate of propagation with an increase of 2 atmospheres in initial pressure, while the arrest in the flame is slightly lowered.

This means the total time in which the whole reaction occurred. The photographs show unmistakably a great rise in the initial rate of combustion due to the effect of the pressure. In further discussion of this point and the interesting conclusions drawn by Mr. Ricardo as to the reason the knock is less at reduced pressures, will Mr. Ricardo explain the behavior of an aviation engine that is, in effect, going up in the air in the altitude chamber? Under those conditions the percentage of the diluent with wide-open throttle is identically the same, irrespective of the altitude, and it is a well-known fact that the detonation dies out with the decrease of the barometric pressure. Also, will Mr. Ricardo give us a better picture of what he believes the phenomena of detonation to be? I believe he has not stated clearly just what he has in mind; I should like him to recapitulate more definitely what his conclusions are with regard to detonation, and state whether what he has said is merely an opinion or whether he has substantiating data. If he has substantiating data, I ask him to present them. He says that, after the combustion proceeds normally for some time, the gas in advance of the flame-front is compressed to a point where it ignites spontaneously and

simultaneously throughout its whole bulk. If this phenomenon does occur, as stated, "spontaneously and simultaneously," how does this set up an explosion wave and, in turn, how can this explosion wave compress the first burned portion to much above the temperature that would be reached by normal combustion?

I do not see the connection between the results obtained from the ignition-investigation device that is described and the internal-combustion engine. Professor Dixon, of the University of Manchester, has done this work in an excellent manner and photographed the behavior of the flame. Those photographs show conclusively that the flame originates at one point, after some little time, and then spreads through the whole mass much as though it had been lit by a spark-plug. In fact, Professor Dixon found nothing to correspond to detonation under those conditions. The difference between this apparatus and the apparatus Professor Dixon used appears to be only from the mechanical standpoint. I fail to understand how conclusions on detonation can be drawn from photographs that fail to show it.

E. R. HEWITT:—I made a large number of experiments for the Bureau of Mines during the war on the premature-ignition point of fuels with some 150 samples of various types of gasoline. The ignition point was determined by pumping the fuel and the proper amount of air into a small, 200-cu. cm. (12.2-cu. in.), bomb. The bomb was then dropped into molten solder. The solder was heated sufficiently to raise the bomb to a predetermined temperature. If the ignition took place then, the explosion was registered by a safety-valve and a gage. By repeating this test two or three times, I could determine the ignition point of any fuel within a few degrees, and there was no possibility of any higher temperature than that determined by the solder when the bomb was dropped in. The conditions were the same as in an engine because the internal section of the bomb was coated with carbon and with oil, as it ordinarily is in an internal-combustion engine. The ignition points varied anywhere from 640 to as high as 720 deg. fahr., that of the usual gasoline being about 670 deg. fahr.

At times, the bomb would not explode. I then drew off the gases through an exhaust-analysis apparatus. To my very great astonishment, I discovered that complete combustion had taken place without any rise of the explosion pressure whatever. I found this would take place in times varying anywhere from 1 up to 8 or 10 min. I then lowered the temperature and found that I could get complete combustion of a mixture of gasoline and air at temperatures as low as 425 deg. fahr. This being an entirely new fact to me, I communicated it to the Bureau of Mines. The chief chemist, Dr. Perdew, verified the experiments that show that these combustions take place at the temperatures as given, and so slowly that there is no rise of pressure. This is exactly the same phenomenon that Mr. Ricardo shows in the diagram in which he gets this flat section where this premature burning takes place before the rise of pressure actually occurs. This seems to me to be a very interesting and very significant phenomenon in detonation. At times I obtained pressures that ran as high as 700 lb. per sq. in., but the usual pressures were about 300 to 500 lb. per sq. in.; so, I obtained detonation in the bomb without any ignition through use of a spark-plug, and the flame wave must have been propagated from all sides at once. There seemed to be no reason the flame should originate at one point any more than any other point.

MR. MIDGLEY:—It appears to me that the drawing of conclusions from the behavior of a bomb of cast iron in regard to what happens inside it is less satisfactory than actually seeing and photographing what takes place, which latter method is the one used by Professor Dixon.

O. C. BERRY:—I am deeply appreciative of the paper presented by Mr. Ricardo. We seldom have a paper that will exert so great an influence on engineering in this Country. Mr. Ricardo has set a striking example for the Society in another respect also. In too many cases the Society is looked upon as an authoritative body, rather than as a forum. We have too much false dignity. We are afraid to express our opinions on a subject unless we feel certain that we are right. The best thinkers maintain a very distinct classification of their ideas. Some things they know for certain; and they have definite ideas about others, with an element of speculation in their conclusions. We ought to descend from any possible pedestal of false dignity and be willing to put before other people the things we have thought about but have not settled, so that they can be discussed. A complete answer will be reached much more quickly than if each fellow thinks about his own problems all alone. I have a great admiration for the man who has sufficient sporting spirit to put before the thinking men of the Country ideas that he himself recognizes are only partly worked-out. I have a great respect for Mr. Ricardo in that he has done this in connection with turbulence; he has admitted to all of us that his conclusions are not complete and that he stands ready to change his ideas somewhat on the subject of the effect of turbulence on detonation. The presentation of a paper of this kind is the finest piece of work that a man can do. I think it should be emulated to a greater extent by the members of the Society.

Mr. Ricardo's paper has again brought to my mind the fact that we ought to come to a standard way of computing engineering data for presentation in papers of this kind. In reading this paper I have found it necessary to recompute nearly all of the results on a different basis so as to make them comparable with my own data. The English base their computations almost entirely upon indicated horsepower, indicated mean effective pressure and the lower heat value of the fuel. In this country many of us base our computations and our lines of thought upon brake horsepower, brake mean effective pressure and the calorimeter or higher heat value of the fuel. It may not matter which of these methods we choose as being correct, but it appears to me that engineers ought to decide upon either one or the other to be used in connection with their public records.

There is a valid reason in the mind of the American engineer for basing computation upon brake horsepower and brake mean effective pressure, in that these are values we all have. Some of us cannot obtain the indicated horsepower very accurately. I have tried to obtain the friction horsepower of an engine by a number of different methods, but I cannot make the results check as closely as I would like. From a theoretical and an analytical viewpoint it is valuable to know the indicated horsepower developed by an engine, but still I feel that there is some valid reason back of the desire of the American engineer to express his results in brake horsepower when presenting them for the consideration of other American engineers.

Mr. Ricardo speaks of the results of preheating the fuel as being the same in the three cases of (*a*) preheating the air that enters the carbureter, (*b*) heating the mixture through contact with the hot valves and cylinder walls and (*c*) heating the mixture through admixture with the hot residual exhaust-products in the cylinder

clearance-space. Properly interpreted, the statement is all right, but an engineer who is trying to make a six or an eight-cylinder engine distribute properly is in great danger of misreading it. The carbureter engineer is not primarily interested in thermodynamic considerations. He uses heat because it enables him to get better distribution and better combustion. If proper distribution is obtained by heating the air entering the carbureter, very high temperatures are required and the power of the engine is reduced correspondingly. Satisfactory results can be obtained with much lower mixture-temperatures if a good hot-spot is used.

Mr. Ricardo's idea of measuring the amount of air used in getting the combustion efficiency of the engine is a line of thought that we have not been making enough use of in this Country. The point is to make it possible to distinguish between differences in carburetion and manifold distribution, on the one hand, and fundamental differences in engine performance, on the other hand. However, it seems to me that the heat liberated per pound of air used should be for full power, rather than for maximum-efficiency conditions. Each pound of air represents the liberation of a greater amount of heat under full-power conditions and, since the engine is actually running under these conditions, it should be charged-up with all of this heat.

CHAIRMAN CRANE:—Mr. Berry mentioned the fact that Mr. Ricardo has admitted that some of his earlier conclusions regarding detonation and its relation with turbulence were not correct. I am always much inclined to accept the evidence of a man who does not claim to be more than 99-per cent right; the man who is 100-per cent right in his own estimation is considerably too good to be true. On the other hand, we have the man who is never wrong, and never being wrong is different from always being right; we recognize that. The man who is never wrong is the man who never arrives at a conclusion until it is supplied by the undertaker.

NEIL MACCOULL:—Approximately what percentages of exhaust gases are present in the combustible mixture at the point where minimum fuel-consumption is shown in Fig. 11 and also at the extreme right of the chart where the compression-ratio is 7½ to 1? The statement is made in regard to piston experiments that the leakage of gas past the piston was measured, but no results are given. Was the leakage measured for all conditions of pressure and temperature and, if a leakage was found to exist under all conditions, how did it vary with the pressure and the possible viscosity of the oil? What was the viscosity of the oil that was used when measuring the piston-friction results shown in Figs. 25 and 26?

HERBERT CHASE:—Will Mr. Ricardo tell what was done in connection with the measurement of the spark-advance, or to determine the effect of the spark-advance upon detonation? Experiments made at the Bureau of Standards have shown clearly that the spark-advance has a very marked effect upon detonation under various conditions, and that this condition should be clearly stated in giving data regarding detonation. What means were used for determining when detonation occurred and when it did not occur? Was a listening test depended upon or were indicator-cards or other means used?

One of the most interesting portions of Mr. Ricardo's paper to me is his data in regard to the stratified or localized charge. The engine used in that type of work is frequently referred to in this Country as a constant-compression engine. A number of persons are experimenting with these engines in this Country to-day. It would be very desirable if these gentlemen would give some data that they have secured as a result of these tests, for comparison with the tests that Mr. Ricardo has reported upon.

STRATIFIED-CHARGE ENGINES

R. W. A. BREWER:—Is Mr. Ricardo's implied condemnation of engines using a stratified charge based on the one type of engine illustrated in his paper, or upon general conclusions of stratified-charge engines?

CHAIRMAN CRANE:—The paper as I read it did not strike me in any way as implying any condemnation of a stratified-charge engine. It simply states the fact known to all of us that the stratified charge holds immense possibilities, but that satisfactory conditions of operation are difficult to reach and that it is not so easy to accomplish through a wide range of powers as it appeared to be at first. I think all will agree that this is a fair reading of the paper and that it is far from being a criticism of the stratified-charge engine.

MR. BREWER:—Referring to the portion of Mr. Tizard's conclusions given in Mr. Ricardo's paper where it is stated that the "rate of burning increases very rapidly with a slight increase of the flame temperature, and whether it will prove sufficiently rapid to produce detonation depends upon the ratio between the rate of evolution of heat by the burning mixture and the rate of heat loss," it appears that those two points govern the whole operation of the engine as regards design from a non-detonation viewpoint and I would like to have some further information on that conclusion.

L. H. POMEROY:—What are Mr. Ricardo's views on the question of carbonization as affecting detonation? It seems obvious that carbonization produces the effect of increasing the temperature of the mixture and also of increasing the compression. It is well known that engines that will run without detonation when clean will show marked detonation effects when carbonized, and it will be interesting to learn whether Mr. Ricardo's experiments have thrown any light on the real effects arising from carbonization. If it is the increased temperature that causes detonation, it seems to follow that there must be some connection between the heat loss during compression and detonation that might make Mr. Ricardo's results not necessarily apply to engines with small cylinders where the heat loss is considerable, particularly at low speeds.

CHAIRMAN CRANE:—I think that all who have had experience with overhauling cars have noticed the fact that, after completely cleaning the cylinders and reassembling the engine, a very considerable increase in the amount of heat being taken care of by the radiator is apparent when the car goes back on the road. That increased heat comes from the fact that we have much cleaner surfaces where the heat is being generated originally.

SLIPPER ALUMINUM PISTONS

G. A. GREEN:—In September 1919 the Fifth Avenue Coach Co. made up a set of Ricardo slipper-type aluminum-pistons. These were subjected to dynamometer and road tests. As a result, some valuable information has been gathered, and it is thought that these data are of sufficient interest to warrant presentation. Fig. 37 shows in a general way the construction of Ricardo pistons. Comparing this design with the conventional heavy-ribbed four-ring aluminum piston used by us, the following points are of particular interest:

(1) There is twice the amount of metal at the crown and back of the rings; ⅜ in. as compared with 3/16 in.

(2) Wristpin bosses are rigidly connected to the crown with 5/16-in. ribs

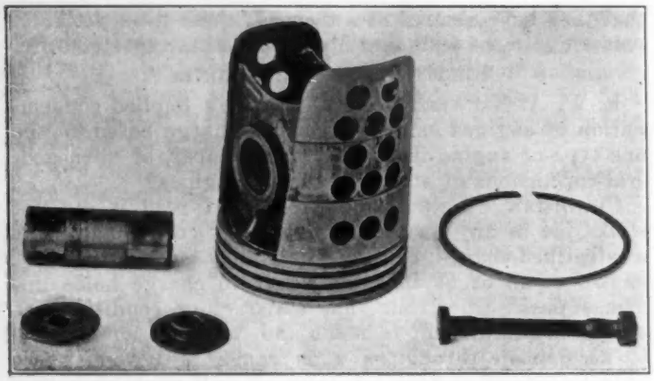


FIG. 37—VIEW OF THE RICARDO SLIPPER ALUMINUM PISTON

(3) Rings are only one-half the width; ⅛ in. as compared with ¼ in. There are three instead of four rings

(4) Slippers are separated from the head by a deep circular cut below the bottom ring. Slippers and head are connected through the ribs carrying wristpin bosses. Because of this separation, it is possible to reduce slipper clearance to the minimum since a considerable amount of the head heat is dissipated before it can reach the slippers

(5) Frictional surface is less than one-half; 17.004 sq. in. as compared with 37.330 sq. in.

(6) Wristpin is well secured by a ¼-in. bolt, nut, two light washers and cotter. The possibility of its loosening-up and scoring the sleeves is remote. This is a very important point

(7) Weight of piston, wristpin and rings is 36.5 oz. as compared with 51 oz.

A standard engine was equipped with Ricardo pistons, put through the usual running-in procedure, and then tested on our dynamometer as follows:

(1) Full throttle, 700 to 1400 r.p.m.; increments of 100 r.p.m. Data taken as to brake-loads and fuel-consumption

(2) Friction horsepower, wide-open throttle, 700 to 1400 r.p.m.; increments of 100 r.p.m. Data taken as to torque required to turn the engine over

On completion of the above tests, the slipper pistons were removed and replaced with our standard type; then the tests were duplicated. Analyzing the results obtained from these tests, it was found that

(1) From the standpoint of mechanical efficiency, there was a difference of 6 per cent in favor of the Ricardo pistons throughout the entire speed-range, the curves being practically parallel

(2) A very slight superiority was shown in regard to fuel-consumption. The difference was, however, almost negligible

(3) The torque developed was higher throughout the entire speed-range, the average difference being 6 per cent. At the peak of the curve, at 700 r.p.m., the amount in favor of the Ricardo pistons was 9 per cent.

(4) The amount of power required to turn the engine over, or friction horsepower, averaged 20.9 per cent less throughout the entire speed-range

Obviously, these results warranted further tests under actual service conditions; so the Ricardo pistons were replaced and the engine was assembled in one of our buses. The road tests commenced in October 1919 and continued until November 1921, when the pistons were removed on account of wear having developed at practically all points. During this period the vehicle covered a service mileage of 65,970 miles. In view of the comparatively short space of time available it was not found possible to prepare complete data as to the performance of the pistons, but the following figures representing the consumption of gasoline and oil from March 8, 1921, to Nov. 4, 1921, are indicative:

Mileage	24,294
Miles per United States gallon of gasoline	7.93
Miles per United States gallon of oil	419

Assuming the use of the Imperial gallon, these figures would be increased to 9.51 and 503 miles per gal. respectively. This can be considered as an exceedingly good

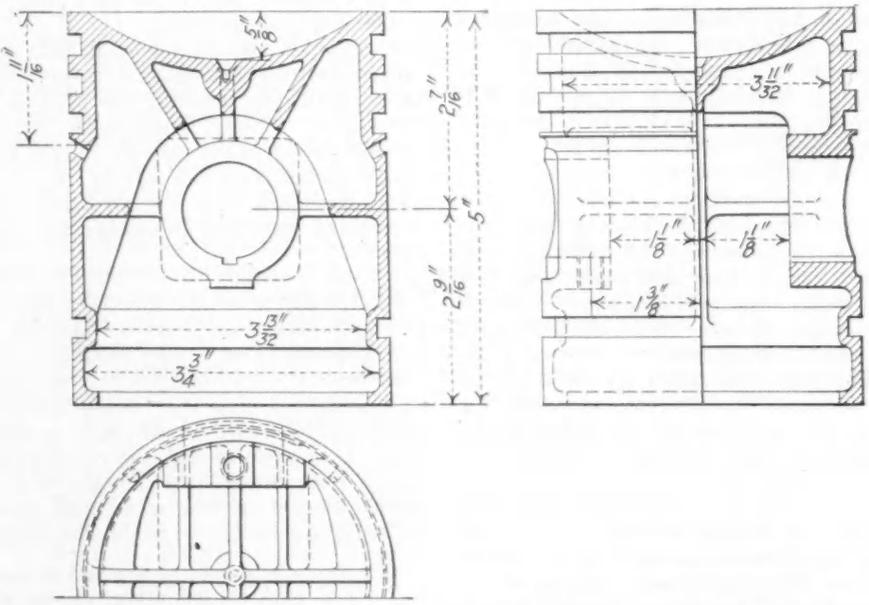


FIG. 38—PLAN VIEW AND ELEVATIONS PARTLY IN SECTION OF THE STANDARD PISTON USED BY THE FIFTH AVENUE COACH CO.

showing from a fuel standpoint, and represents approximately 54 ton-miles per United States gallon throughout the entire period. On completion of the test the engine was stripped-down and the pistons, sleeves and cylinder-heads examined from the standpoint of lubrication. The following points were observed:

(1) There was almost perfect inner sleeve lubrication
(2) The slippers and surfaces between the rings were clean and free from carbon
(3) There was a rather heavy deposit of carbon on the under side of the heads
(4) There was a very slight deposit of granular carbon on top of the pistons; the heaviest deposit was nearest the exhaust-ports
(5) There was practically no carbon deposit on the junk-heads
(6) The junk-rings were in excellent condition, showing nearly 100 per cent bearing surface and perfect lubrication

The pistons and rings were then carefully gaged to determine the amount of wear. The results of these measurements are shown in Table 11.

TABLE 11—WEAR OF PISTONS AND PISTON-RINGS

Location	Wear, in.
Above Top Ring	0.0060
Between Rings Nos. 1 and 2	0.0070
Between Rings Nos. 2 and 3	0.0065
Top of Slipper	0.0130
Bottom of Slipper	0.0100
Diameter of Rings	0.0015
Width of Rings	0.0015
No. 1 Ring Groove	0.0110
No. 2 Ring Groove	0.0060
No. 3 Ring Groove	0.0030

Analyzing these figures, it will be noted that

(1) The wear between the rings is fairly constant and averages 0.0065 in.
(2) Wear at slipper is 0.003 in. greater at the top than at the bottom. The average wear is 0.0115 in. This, plus the necessary clearance, was sufficient to produce considerable noise
(3) Wear at outside diameter of rings, 0.015 in., is not unreasonable considering the mileage
(4) Wear at sides of rings, 0.00075 in. per side, is not unreasonable considering the mileage
(5) It is interesting to note the progressive wear of ring grooves, 0.003 to 0.011 in. The top groove shows up worst

Due to the theoretical advantages of this class of piston, one would naturally expect decreased vibration, piston slap, friction horsepower and fuel-consumption, and less wear-and-tear generally; in short, a better all-around engine performance. In actual practice, all of these points were proved. At the beginning, it was thought that perhaps considerable trouble might be experienced from rapid wear and high oil-consumption. However, this fear proved to be groundless.

Answering the points raised by Mr. Crane, there are shown in Fig. 38 various views of our standard piston. Judging by the experimental set referred to in this test, there should be very little difference between these pistons from the standpoint of wear but, of course, one instance does not prove this. As a matter of fact, there seems no good reason why the Ricardo piston should not have equal wearing properties. The same remarks apply insofar as cylinder wear is concerned. Of course, it must be remembered that with Knight engines the inside diameter of the inner sleeve is the only point at which wear occurs as a result of piston movement.

Perhaps it is scarcely necessary for me to enlarge upon the great value of properly carried out research work. Many of us have learned slowly and painfully that lasting success, mechanically speaking, can be attained only when there is a proper coordination of theory and practice. I am sure we are all agreed on this subject: the establishment of the Society's Research Department represents the stamp of official approval of this. In our own research department we have collected much very useful information, and we are prepared in practically all cases to place our records at the disposal of members of the Society. It seems to us that a broad-gage policy such as this must in the long run be helpful, since it will foster a spirit of practical cooperation that should be of benefit to the industry. Unquestionably Mr. Ricardo's paper represents a most valuable contribution to the industry.

E. A. SPERRY:—This paper covers but a fraction of Mr. Ricardo's contributions to this wonderful art that appear from time to time. Inasmuch as the subject is research and research has to do with the future, why should we not take one other aspect of the future with regard to detonation? Here is an example where automotive engineers have apparently taken to the woods in trying to dodge detonation. As a matter of fact, how do we know that Nature is not trying to hand us something advantageous in detonation? Why not seek to harness it? One absolute fact has been established; it is that we know we get higher efficiency when we get detonation. That has been the result of observations on both sides of the Atlantic. Why do we not design an engine around the proposition of detonation? Why dodge it? Think for a moment what this would accomplish. It would relieve us entirely of all these troublesome questions of stratification and all of this perplexing matter of fuels. What would we care about fuels in such a case? Not only could we use the present-day gasoline, but we could use kerosene and go on to the much cheaper fuels. Engines are running today on fuels that are quoted on the present market at $1.22 per bbl., with clear and odorless exhaust. Let the fuel detonate; the more it detonates the more efficiency it provides. That would relieve us entirely of compression-ratio limitations. We then would be able to increase the compression and get the higher efficiencies to which we are entitled. It would relieve us of carbon deposits by permitting us to increase combustion temperatures to a point at which no carbon is ever deposited. It would relieve us of all the trouble due to carbon monoxide, since not a particle of that gas is formed when fuels are burned at high temperature. Thousands of Diesel engines work at these high temperatures without any trouble. Steam engineers now seek Diesel-engine jobs on ships. They find that it is easier than steam-engine work, with its boilers and endless auxiliaries.

H. S. MCDEWELL:—All technical and scientific publications appearing in Great Britain seem to attach considerable significance to dissociation. About what percentage of influence does dissociation have in the results of Tizard and Pye? Assuming a maximum combustion temperature, which Mr. Ricardo has mentioned in a previous paper as being 2480 deg. cent. (4496 deg. fahr.), only about 0.2 per cent of the weight of the hydrogen present in a normal gasoline would be dissociated. This would correspond to about 0.6 per cent of the heat of combustion of the fuel; consequently, that does not seem to be a very material effect. How closely do Tizard

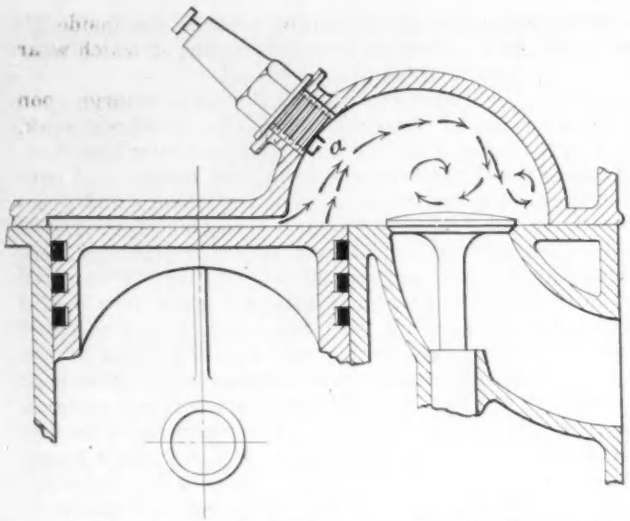


FIG. 39—LOCATING THE SPARK-PLUG AT *a* WAS SUPPOSED TO GIVE THE MAXIMUM TURBULENCE IN A VALVE-IN-HEAD ENGINE

and Pye's results check with these figures? Does the good luck that Mr. Ricardo mentions in connection with his efforts with a stratified-charge engine also extend to his supercharging engine?

H. L. HORNING: — Mr Ricardo's paper cannot be absorbed within the short time we have had to read it. We have been quite unfair in asking him to expose his innermost thoughts on the entire range of subjects in the experimental development of the internal-combustion engine. A man can be an expert on one or two phases of any subject, but when we ask him to cover all of the details of a general subject, it becomes a very difficult task. Measured by that standard, he has met our expectation marvelously.

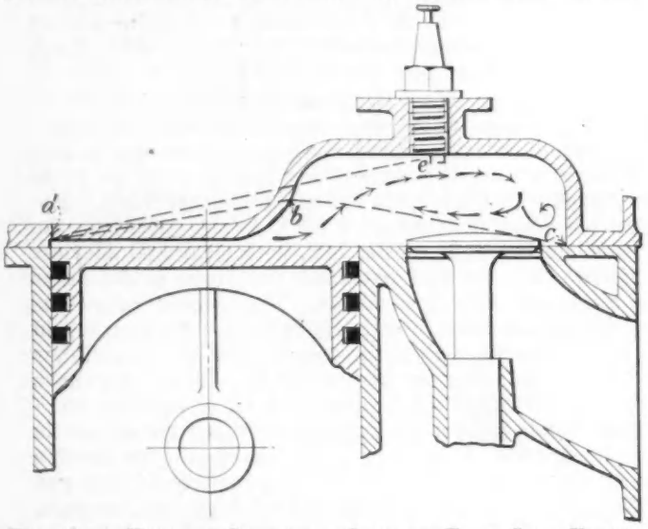


FIG. 40—A FORM OF COMBUSTION-CHAMBER THAT GAVE VIOLENT DETONATION

In trying to trace the relationship between detonation and turbulence in the Ricardo shape of combustion-chamber, J. B. Fisher, the chief engineer of the company with which I am associated, and I agreed that if we located a spark-plug at the place of maximum turbulence, we would certainly kill all detonation. According to our best information, we located the spark-plug as shown at *a* in Fig. 39, but detonation continued.

Mr. Ricardo's most effective combustion-chamber is the form shown in Fig. 40. We used this form and the engine detonated violently. Mr. Fisher called attention to the fact that right at the point *b*, as the piston comes up, there must be a terrible movement of gases. There was no other way to get more effective turbulence because, at the moment the spark jumped, we were getting the most violent passage of the gases. We located the spark-plug at *b*, but that did not help matters except that it showed by that simple experiment the correctness of what Mr. Ricardo has told us. In his paper Mr. Ricardo has called attention to the fact that it is the maximum distance *bc* or *de* that almost certainly seems to be the controlling factor. I have talked with Mr. Ricardo and

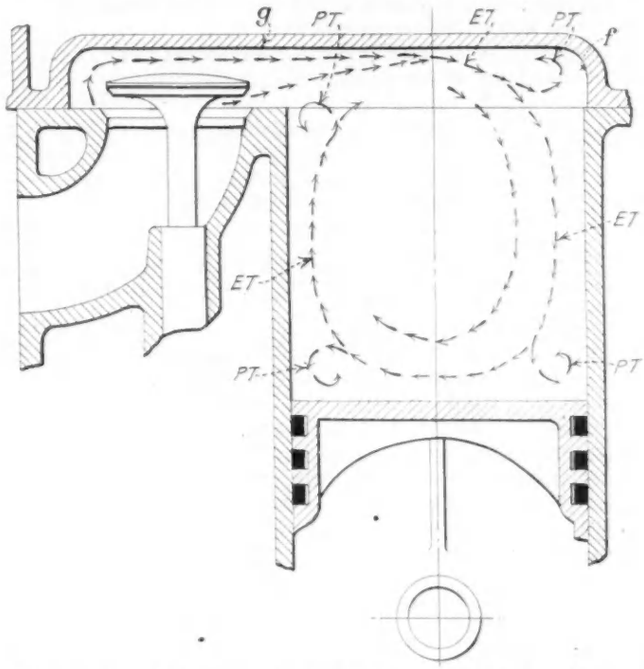


FIG. 41—AN ORDINARY TYPE OF L-HEAD COMBUSTION-CHAMBER SHOWING TWO POSSIBLE LOCATIONS FOR THE SPARK-PLUG

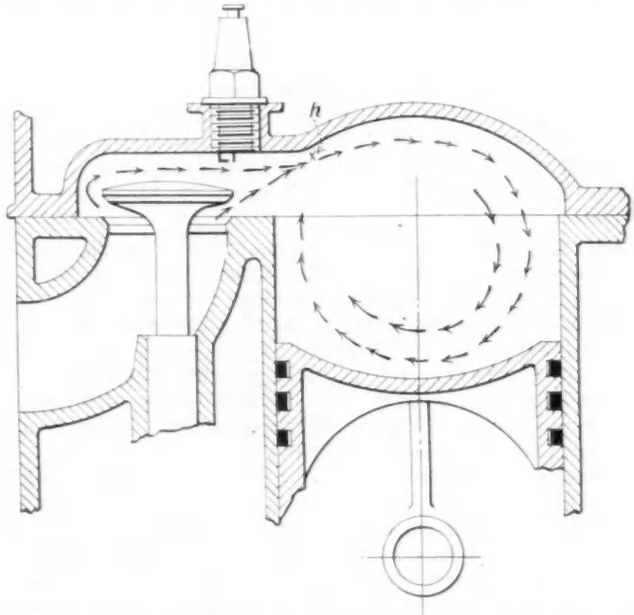


FIG. 42—COMBUSTION-CHAMBER OF THE MARK WEBBER TANK ENGINE DESIGNED BY RICARDO IN WHICH LOCATING THE SPARK-AT *h* DID NOT GIVE GOOD RESULTS

we agree that there is one other factor; it is the tendency to detonate when the spark-plug is opposite to a pocket. I wish to call attention to the fact that if the spark-plug is located at *f*, as in Fig. 41, that would obviously increase the distance and the spark-plug would be opposite the valve-pocket, but to locate it at *g* would decrease the distance and the spark-plug would be within the pocket.

Mr. Ricardo has referred in his paper to remarks that I made in my paper on Turbulence in regard to his Mark Webber engine. We made a test with the spark-plug located as at *h* in Fig. 42, and could not get good results. I said in my paper that Mr. Ricardo seemed to get good results, but he says now that they had trouble and had to move the spark-plug to the location shown at *h* in Fig. 42. Those two factors constitute the evidence in support of his statement that turbulence does not affect detonation directly.

F. E. MOSKOVICS:—If the members of the Society will take the paper presented by Mr. Ricardo as a mental stimulus in addition to the value of the actual data it contains and the original research it discloses, they will derive the maximum benefit from it. I trust and believe that this paper is only the beginning of a discussion that will be general and of long duration. This is the stuff that the advancements in science are born of, and in that regard Mr. Ricardo's paper is peculiarly timely and forceful. May we at the same time convey to him the thought that, in the discussion, however freely views may be discussed, nothing personal is intended; it represents only the true zeal of the scientist in reaching the goal. Mr. Chairman, in view of the peculiar timeliness of Mr. Ricardo's paper, I move that the members of the Society express, by rising, a vote of thanks to the distinguished visitor, Mr. Ricardo, for traveling so far and going to such pains to present this remarkable paper to the Society. [A rising vote of thanks was accorded Mr. Ricardo, accompanied by hearty applause.]

CHAIRMAN CRANE:—Having thanked Mr. Ricardo for his paper, we now incur a further obligation by requesting him to answer some of the questions that have been asked.

MR. RICARDO:—Thank you very much for showing so much interest in my paper and for the very interesting discussion.

Mr. Grimes raised a point regarding the value of the exhaust products in a kerosene engine. I am not quite clear what exactly he has in mind but, rightly or wrongly, I understood him to mean that he had obtained equally good results by using a lower compression-ratio. Insofar as power output is concerned, I fully agree; because, from this point of view, there is very little difference. With a low compression-ratio you retain a greater proportion of exhaust products within the cylinder; with a higher compression some additional products must be added to keep the flame-temperature down. These additional exhaust-products reduce the volumetric efficiency so that the increase of thermal efficiency due to the higher compression and the loss of volumetric efficiency due to the diluent just about balance. The power output remains therefore the same whether the compression be high or low within reasonable limits, of course. The thermal efficiency, however, is dependent directly upon the compression-ratio and from this point of view the use of additional exhaust-products and a high compression are preferable up to a certain point. I gave the results of some investigations on these lines in Fig. 11 of my paper. The instance Mr. Grimes cites, of an engine running steadily at 2250 r.p.m. at nearly full throttle and failing at a very slight increase in throttle-opening, is very interesting as showing that a very slight addition to the charge and therefore to the flame-temperature will bring about detonation, increased wall-temperature and subsequent preignition. This is just what one would expect.

Mr. Wilson raises the question of the influence of pressure on the rate of burning. I need hardly say that his remarks are of special interest to me. I had always clung to the conviction, being no chemist at all, that the pressure plays much the most important part, but I was rather shaken out of that idea by our chemists in England. I gather from Mr. Wilson, who, I recognize, speaks very authoritatively, that it may play a very significant part indeed. This being so, all I can say is that I am very pleased to hear it because it supports my own feeling which I had rather surrendered on account of the criticism leveled against it. I do not think that it affects Tizard's more recent theory except, perhaps, in a quantitative degree.

Mr. Midgley raises exactly the same point; that the pressure does play a part. I am very glad to hear this view supported, for I am still inclined to believe that it does. The altitude question has always been my own pet argument in support of the pressure theory; so we are in complete agreement there.

With regard to Professor Dixon's results, I agree that results yielding photographs as his did are very much more valuable. I wish we could have taken photographs in this case, but the experiments were carried out on a small apparatus where the cooling losses were playing a very serious, if not overwhelming, part. Mr. Tizard was asked by the Board of Industrial Research to go carefully into these results and see if it was not worthwhile trying the experiments on a larger scale; so we used a cylinder which was precisely the same size as that of the variable-compression engine with which we were carrying on the other tests. That is really the reason for it. It became very apparent early in the tests that the rate of dissipation of heat was playing a very vital part.

Mr. Midgley asked if I would give the intermediate stages between the results from the compression machine and the conclusions on detonation deduced from them. I wish I could. I had hoped before today to be able to do so, but those deductions form the subject of a paper that Mr. Tizard will publish very shortly; therefore, I must ask that his paper be accepted as the intermediate stage when it appears. I had hoped and expected that Tizard's deductions would have been published before now, when I should have been free to quote them.

Mr. Midgley further criticizes, and very justly too, the use of the words: "spontaneously and simultaneously." These words must of course be interpreted as relative. In this connection I should like to say that I had no opportunity to correct the preprint of my paper and that there are, I fear, a number of errors in it. In the copy I read from, the word "nearly" was introduced and I was careful to emphasize this when reading the paper. As I interpret Dr. Dixon's photographs, it seems to me that he did find almost exactly what is generally meant by detonation, namely, an extremely abrupt change from a normal rate of burning to that of an intensely rapid one.

Mr. Hewitt gave some very interesting details of his experiments with a bomb. His results are almost exactly the same as were obtained under sudden adiabatic compression and are therefore particularly interesting to me. We also had cases where combustion was complete without any appreciable rise in pressure, but I do not think

there is anything surprising in this. We assume that the rate of combustion is a function of temperature, but that it rises very rapidly with increase of temperature. Combustion undoubtedly takes place at comparatively low temperatures, but the rate is then very slow indeed.

Mr. Berry mentioned the advisability of expressing results in terms of brake horsepower, rather than of indicated horsepower. I have adhered consistently to the indicated performance for the last 2 or 3 years. I quite agree that there is a possible error in determining indicated horsepower; but, by adding up the friction losses obtained in various ways and by various cross-checks, I think one can obtain indicated horsepower pretty closely, at all events in the case of engines with which I have had wide experience. I prefer it, because the results expressed in indicated horsepower are applicable to any case; whereas results in brake horsepower are really peculiar to the engine one is using. If I had expressed the results in terms of brake horsepower in our particular variable-compression engines, I should have given rather scandalously good results because the mechanical efficiency of that engine is exceptionally high. It is a ball-bearing job throughout. I particularly wanted to avoid that. Those results would not be applicable to other engines.

Mr. Berry refers to the division I made as to the sources from which the entering charge receives heat. I ought perhaps to have emphasized that I was referring only to the question of volumetric efficiency. Only the first of the three sources is of any interest from the point of view of distribution; the others may, I think, be regarded as constant. With regard to air-measurement tests and the use of them for deducing the efficiency, I think that the point he raised is dealt with in Appendix 3. I should like to take this opportunity of thanking Mr. Berry for his very flattering and sympathetic remarks.

Mr. MacCoull asked for figures as to the percentage of exhaust products used in connection with Fig. 11. I cannot quote it exactly because it was not measured directly. We had no means of measuring it at that time, but a fair estimation can be obtained from the difference between the mean effective pressure obtained with and without; that is, between the mean effective pressure obtained with pure benzene and the mean effective pressure obtained with gasoline and exhaust products. That is only a rough approximation because, of course, the efficiency was not the same in the two cases. Without going into great detail, I should think that, if the ratio of mean effective pressure is taken in the two cases and a further allowance of 50 per cent for the drop in efficiency is made, this will approximate the proportion of exhaust products.

With regard to leakage of gases and the quality of the oil used, I am not, I regret to say, provided with any figures. We have made no leakage tests for a long time; in fact, not since the war. We used Price's gas-engine oil in all cases. It is an almost uniform standard oil in England that we used to employ throughout all our testing; but I have no figures, I am sorry to say, as to its viscosity. I certainly found that the viscosity of the oil played a very important part in piston gas-leakage.

Mr. Chase inquires about the degree of spark-advance used in determining detonation. I think that is a very important point. We have always defined the detonation point as the highest compression-ratio at which one can advance the spark without detonation until the torque definitely falls. For example, at a compression-ratio of 5 to 1 and with average gasoline, the maximum torque is obtained with an ignition advance of 32 deg. before top-center. If we advance to, say, 40 deg. before top-center with a mixture giving complete combustion, the torque falls considerably and, if we can get over to that point without detonation, we consider we are within the range with every change in compression-ratio. The whole available range of ignition timing is explored in this manner.

As regards the method of detecting detonation, we have tried various devices, including the optical indicator; but, in the case of this particular engine, we have found the listening test a very sensitive one, for the cylinder appears to be exceptionally resonant and detonation is very definite. We have tried with various observers on a standard fuel and three different observers working independently and at different times have determined the maximum compression-ratio as being 4.83, 4.85 and 4.85 to 1 respectively. Again, in this engine, detonation, if allowed to persist, very soon breaks down into preignition. It is only since I have been here that I have heard for the first time of the very ingenious device which Mr. Midgley has been using and which appeals to me very strongly.

Mr. Brewer suggested that I had rather damned the stratified charge. I am sorry if he got that impression, because it is not the one I intended to convey. I have always been, and I am still, a great enthusiast for stratified charge. I did not want to paint it in too rosy colors, because I have had some very unfortunate experiences with it, but I certainly think it has a great future.

Mr. Pomeroy raised the question of the effect on detonation of carbon on the piston. It is rather difficult to give what experience we have in any quantitative terms, but the effect of carbon on any part of the combustion-chamber is to delay the rate of dissipation of heat. This is very conspicuous in the case of the detonation or compression machine. There, if one had bad detonation or if one worked with a very rich mixture, a thin coating of carbon would form on the walls with the result that the next experiments would be very erratic indeed and show a large difference in the temperature coefficient. It was necessary, therefore, always to clean and wipe out the cylinder thoroughly. As regards quantitative terms, in the case of the variable-compression engine, when it is quite clean, it will run on a straight paraffin gasoline with a compression-ratio of 4.85 to 1. When it has run for about 3 months at an average of 5 or 6 hr. per day, that ratio has dropped to about 4.75 to 1. We have to make a correction; we retest from time to time on it. After about 3 months the engine is taken-down and recarbonated.

Mr. Green gave some data that are very interesting to me on the piston he has been using. I have nothing to add except that his contribution to the discussion is very gratifying indeed, and I am most grateful to him for the trouble he has taken.

Mr. Sperry, as I understand him, was putting forward a strong plea for the Diesel or high-compression self-igniting engine for automotive work. While I sympathize with all his arguments, I feel that this is rather a long-distance shot as yet. I cannot claim a great deal of experience. I was watching the British Admiralty's experience on Diesel work throughout the war. It seemed to me that considerable advances were gained, but there is still a very long way to go before we can consider that as a possibility. I feel that the use of a stratified charge is an intermediate measure. It is, in a sense, getting a Diesel cycle, although it does not, of course, permit of

(*Concluded on page* 347)

The Tractor Market

By J. S. Clapper

Minneapolis Section Paper

AFTER stating that the tractor industry has reached the most critical period in its history and that the farmer has been quicker to appreciate the possibilities of applying mechanical power to farming than the tractor builder, in that the latter has held to producing a one-purpose machine instead of analyzing farm demands and producing a machine adaptable to them, the author outlines diversified-farming requirements and supplements this with statistical quotations from a Government report that indicate the character and exent of the market for tractors and advocates a machine suitable for all farm purposes.

Farm-implement finance is considered briefly, and tractor suitability for specific purposes is commented upon, inclusive of statements by representative tractor distributors.

THIS subject is of such importance and covers such wide possibilities for argument that, in trying to analyze the conditions that have affected the tractor market, I may subject myself to criticism from some tractor builders. This is especially true when I say that the tractor business has reached its peak and that it will be a long time before the farmer can be persuaded to accept the present type of machine and to do his part in motorizing his farm, unless some changes are made by the builders, and that very quickly. I will not attempt to tell how tractors should be marketed, but shall mention some of the vital things that I believe are affecting the demand.

There is too wide a gap between what the builder receives for his product and what the farmer is compelled to pay. The former does not receive enough money to meet, later on, the demands made by the selling organization and the user. My opinion is that the tractor industry has reached the most critical period in its history and whether it goes forward or backward depends entirely upon the producer and the sales organization through which all products must find an outlet. We must all admit that the tractor business is in a state of depression, but I do not know how many of the companies have made the proper analysis of the conditions and are willing to apply the proper remedy. I do know that some of the large manufacturers of accessories necessary to the production of tractors, as well as the managers of some of the most prominent advertising mediums, are fully awake to the situation and are trying to place some of the actual facts before the builders. We all know that it takes more courage to correct a mistake than it does to suffer for years on account of it. From observation, I am thoroughly convinced that the farmers have been quicker to appreciate the possibilities of applying mechanical power to their farm operations than the manufacturer. I base this statement upon the fact that the latter, as a whole, have held to a "one-purpose" machine, expecting the farmer to arrange his work to fit the machine instead of analyzing the farmer's requirements and supplying him with a machine that will fit his farm work. I think this has resulted mostly from following the old custom of designing and perfecting a machine or implement to do one certain class of work.

[1] M.S.A.E.—President, Toro Motor Co., Minneapolis.

In the early days of the development of the tractor, only two operations were considered; first, the breaking-up of the large acreage of prairie sod and, second, the pulling of separators for threshing or the use of the tractor for providing belt-power. I often wonder if the manufacturer of farm implements has ever given the farmer the credit to which he is entitled for the part he has played in developing and perfecting every farm implement that has been put on the market. It was the farmer who first discovered that disc-harrows, a battery of seeders or grain-binders could be pulled successfully by a tractor.

Diversified-Farming Requirements

No one who has any knowledge of farming with horses in the section devoted principally to grain and has seen horses replaced by a well-designed tractor can question its adaptability to such service and the wonderful saving it effects. It was in this field that the present type of tractor was developed and found such a ready market; but this field is small in comparison to the entire acreage of crops that must be cultivated. A tractor designed so that it can be used for plowing, preparing the seed-bed, planting and cultivating will meet all of the conditions and requirements in the diversified-farming sections, and it will prove more efficient and economical than horses. There are five prospective purchasers for a machine of this type where there is one purchaser for the one-purpose tractor.

Government statistics show that there is a total annual loss of $232,644,000 on account of weeds in corn, cotton, potatoes, beans, sugar beets, sweet potatoes and soy beans, due to lack of proper cultivation. It has been my firm conviction for several years that it is entirely practicable and feasible for engineers and builders to analyze the farmer's requirements correctly and hold their design and actual accomplishments to fit the work to be done. It has been demonstrated thoroughly that a machine can be made strong enough and have ample power to handle two plows, but still be light enough for all classes of row-crop cultivation from sugar beets to corn. However, the engineer or company that has not had actual experience in the cultivating of row crops should study every operation and become thoroughly familiar with the requirements first.

The most difficult operations in farming are the first and second cultivations of the tender plants and, unless the operator has an entirely unobstructed view of the rows and the machine has enough flexibility that the cultivating teeth of the shovels will respond promptly to every controlling movement of the operator, good clean cultivation is not possible without injury to the plants. Unless we can give the farmer a machine capable of doing equally as good cultivating, one that is easier and more economical to operate and will perform the work faster than it can be done with horses with less effort on the farmer's part, we have but a slight basis of argument to persuade him that he should motorize his farm.

What argument can a salesman present to the farmer today that will persuade the latter to invest in a machine,

TABLE 1—FARMS OPERATED BY THE TRACTOR IN 1920

State	Per Cent
South Dakota	16
North Dakota	15
Montana	12
California	10
Kansas	10
Illinois	9
Iowa	9

at the present prices of horses and feed, when the records show that tractors have replaced only 20 to 22 per cent of the horses? What would be the result if another salesman could show this same farmer a machine so adaptable to his work that it would actually replace 75 to 80 per cent of his horses and 40 to 60 per cent of his surplus labor, if the price of the machine were within his reach? The machine must be capable of ready adaptability to the different classes of work and be sold at a price within the farmer's purchasing power, which has now reached the lowest point in his experience. The all-purpose machine designed so that it can be adapted easily to more different kinds of work on the farm is entirely practical and will be accepted readily by the farmer who is searching for improved and economical methods. No one engaged in the tractor business is more optimistic over the possibilities of power farming or the success of motorizing the farm than I am, but we must first motorize our ideas, our factories and our own products before we can expect the farmer to motorize his farm.

TABLE 2—ACREAGE OF THE FOUR PRINCIPAL CROPS

Crop	1910 Acreage	1919 Acreage	Difference, acres
Corn	69,000,000	63,000,000	6,000,000 (Decrease)
Wheat	32,000,000	53,000,000	21,000,000 (Increase)
Oats	27,000,000	31,000,000	4,000,000 (Increase)
Cotton	14,000,000	15,000,000	1,000,000 (Increase)

Statistical Consideration

On Jan. 1, 1920, the Department of Agriculture reported a total of 246,000 tractors on the farms in the United States or about one tractor for every 28 farms. The States showing the highest percentages of all farms reporting tractors are listed in Table 1.

The complete report covering the year 1920 gives the total valuation of all farm equipment produced in that year as $537,000,000. The report shows further that 203,000 tractors were constructed in 1920. Their value was $193,000,000 or more than one-third of that of the total of farm equipment.

Acreage of the Four Principal Crops

Let us consider the acreage of the four principal crops for 10 years as shown by the Government statistics given in Table 2.

These figures indicate that there has not been a very great increase in the acreage of the four principal crops during this period, but other statistics show that the value of farm-implement equipment during the same period increased 185 per cent. It has been the custom of the Department of Agriculture to tabulate each season the totals of 22 of the principal crops, one-half of which are row crops requiring cultivation. The figures in Table 3 are given by States, indicating the difference in valuation of these 22 crops produced in 1919 and 1920.

TABLE 3—PRINCIPAL CROPS OF 1919 AND 1920

State	Decrease in Valuation of 22 Crops	Decrease in Valuation of Total Crop
New York	$23,000,000	$32,000,000
Pennsylvania	50,000,000	77,000,000
Maryland	18,000,000	23,000,000
New Jersey	3,000,000	6,000,000
Ohio	173,000,000	199,000,000
Illinois	344,000,000	400,000,000
Iowa	263,000,000	450,000,000
Minnesota	197,000,000	212,000,000

The loss in the Middle and Western States is much larger than in the New England States. This is an indication of the shrinkage in valuation that the farmer has been forced to accept recently.

From 1914 to 1918 New York State ranked twelfth in respect to the total valuation of crops. In 1919 it was the fourteenth State, but in 1920 it held fifth place. From 1914 to 1918 Pennsylvania ranked fifth and in 1920 dropped to ninth place. For the same period Illinois was first in respect to all crops and in 1919 dropped to third place. Minnesota was ninth during this period and dropped to seventeenth place in 1920 in respect to all crops. The number of tractor companies and the sizes of machines built in 1920 are stated in Table 4.

TABLE 4—NUMBER OF TRACTORS BUILT IN 1920

Horsepower	Number of Companies	Number of Machines
8 and less	6	7,678
9 to 15	8	3,366
16 to 18	17	107,782
19 to 22	17	39,964
23 to 26	24	18,073
27 to 32	33	19,861
33 to 39	10	1,410
40 to 59	15	3,684
60 and over	13	1,389
	Total	203,207

Of the total of 203,207 machines manufactured, only about 29,000 were estimated as having been sold for export, leaving 175,000 for domestic trade. The total value of all farm equipment produced during 1920 was $537,000,000 and over $500,000 of this amount represents horse-drawn cultivating tools. Only $66,000,000 of this was estimated as having been sold for foreign trade, leaving $471,000,000 for domestic trade.

Our export business in all lines of farm equipment declined during the war, but production was normal except in 1918 and our surplus that formerly went to Europe was accumulating in this Country. Also, with our enormous increase in production during 1919 and 1920, caused by the urgent appeal of the Government officials, it is only reasonable to assume that we would have felt the effects of over-production in the farm-equipment line even if the farmer's product had maintained its price level. If one consults the records, it will be found that whenever our imports increase and our exports decrease so that the imports exceed our exports in value, we feel the effect in all lines of business. Not until our surplus begins to move to foreign countries will business conditions improve to any marked degree.

Farm-Implement Finance

Much has been said and written about convincing the country banker of the value of farm tractors. Perhaps the people who feel that the country banker has not been convinced do not consider the matter from the banker's standpoint. One of the great troubles throughout the entire Country today is that the banker has been oversold. The country banks are not in a position

to extend further accommodation to the farmer until he has liquidated some of his present debts.

The principal business of the average banker is to accumulate in the bank's vault as much money as possible. This money must be sold to the borrower at a profit. The money does not belong to the banker; it belongs to the depositors and the law compels the banker to safeguard the interests of the depositors to the best of his ability. His only means of making money is to sell his depositors' money to the best advantage. He must not only have good security but must know that the borrower has other resources. No banker wants to resort to foreclosure to realize on the security given for a loan. The banker knows from experience that it is perfectly safe to loan a farmer money to buy horses, cattle or seeds and, if he finds it necessary to resort to the sale of his securities for any reason, he can realize the amount of the loan. There has always been a stable market for livestock; hence, the banker takes little chance of losing his money.

On the other hand, a majority of the farmers who desire to buy a gasoline tractor on credit expect the banker to take the tractor only as security. The banker must judge whether the tractor will be a profitable investment for the farmer. If the banker does not think the tractor will prove to be a profitable investment for the farmer, he refuses to loan him the money. I believe that all good bankers are ready and willing to loan money to any farmer who is responsible and entitled to credit; that is one of the banker's means for making money. The country banker has been criticized too severely for not financing the farmer in his purchases of tractors.

Tractor Suitability

In a recent report on tractors sent out by one of the largest advertising mediums it is shown conclusively that the broad level prairies are the best field for tractors. This indicates that the one-purpose tractor has proved successful in the grain-growing sections or the prairie country devoted strictly to grain, but the company making the report questions whether the present type of tractor has proved successful and economical in that part of the country where row crops are grown to a large extent, or on the smaller farms. This advertising firm does not doubt that we are a long way from having convinced the farmers of the practicability of motorizing their farms; in other words, it feels that the farmers are not thoroughly convinced of the value of power farming and will not be so convinced until the tractor builder produces a machine capable of doing really all of the farm work that is performed at present by horsepower. Another company sent out inquiries and of the replies from 400 farmers in sections where the principal crops are corn, cotton and other row crop, about 90 per cent can be classified as follows:

(1) Not fitted to general farm operation
(2) Tractors should be strong enough to plow and light enough to cultivate
(3) Cannot afford to buy a tractor for plowing and keep horses to cultivate

A bulletin recently issued shows that in New York State 252 farmers who own tractors were able to reduce their horse equipment 22 per cent. During October and November 1920, representatives of the United States Department of Agriculture visited 286 tractor owners in Ohio, Indiana and Illinois. On the average the tractors were used for 30.8 full days of the year. While 85 per cent of the plowing, 75 per cent of the discing and 41 per cent of the grain-cutting was done with the tractor, the cultivating of corn is the operation that requires the greatest amount of horse-labor in the shortest time on the farm.

Tractor Distributors' Opinions

I have received letters from three of the largest distributors of tractors, who have had years of experience in this line and know whereof they speak. I will quote them for further evidence of what is necessary to improve the tractor business. In a letter dated July 1921, one of them said that the company had not sold the number of tractors that it should have sold this year. A special agent of one of the large tractor-producing companies stated that the motor cultivator supplies the missing link in territory where row crops are produced, and that this would allow his company to sell tractors to farmers who formerly had refused to purchase them because they did not replace horses for the cultivation of the crops. This agent said also that in his opinion a large tractor is necessary on the large farm but that it is debatable whether such a tractor is profitable without the cultivator. The distributing company feels that with the convertible machine any farmer who tills 160 acres will find it a profitable investment because it can be converted from a tractor to a cultivator and will do practically all of the work on a farm of this size. An instance was given of a young man who, while a motor cultivator was being demonstrated, drove it about six rounds and did nearly three times as much work as his father and brother did together with their horse-drawn cultivators in the same time.

Another company stated in June 1921, that it was so firmly convinced that farm tractors are not adapted economically to the corn belt that it intended to discontinue their sale. The view was expressed that there is very little question that a tractor will replace very few horses and men on the average-sized farm, of say 160 to 200 acres, unless some means is arranged for taking care of the cultivation of the corn. Further, that the corn farmer must keep enough horses and men to cultivate his corn and that under these circumstances he has practically all the power he requires except for belt purposes; that it is therefore plain that the average farm tractor in the corn belt is fundamentally wrong in that it is not built along lines that cause it to function economically; and that it has been developed as far as it can be developed unless some method of handling the corn cultivation is incorporated into its mechanism, although there is no question that the present type of farm tractor is suitable for use in the wheat fields in the Northwest and in Western Canada.

In June 1921 the third tractor-distributing company wrote that it had done too much toward selling the farmer on the word "tractor", rather than selling him on a basis of design and performance. This company also stated its belief that the all-purpose machine is the one farm-power agent that will stand out as a real factor in the solution of general farm-power problems, because it is theoretically, practically and economically suitable.

Use of Molybdenum Steel in the Automotive Industry

By JOHN D. CUTTER[1]

DETROIT SECTION PAPER *Illustrated with* CHART AND DRAWING

THE author discusses the advantages of straight chrome-molybdenum steel, because it has the widest field of usefulness, and carbon-molybdenum steel in sheets for pressed-steel parts. Reference is made to a bibliography of test results and similar data. The use of molybdenum steel is first considered in connection with the light-weight car. The adaptability of this steel to frame construction and its use for other parts also are commented upon, inclusive of manufacturing costs, and several tables that embody comparative data are presented.

Three specific divisions of advantages gained through use of this steel are stated, and quotations are made under each division from reports and statements of metallurgists and manufacturers in support of these claims. The author believes that the use of molybdenum as an alloying element for steel will have a favorable effect on the economic status of the automotive industry.

MANY types of molybdenum steel have been produced. Some of them contain nickel or vanadium in addition to chromium and molybdenum. These exhibit extremely high physical properties but, when commercial factors are taken into account, it is apparent that the straight chrome-molybdenum variety has the widest field of usefulness and it is this type of steel, together with carbon-molybdenum steel in sheets for pressed-steel parts, that will be considered. The subject divides itself naturally into a discussion of the opportunities molybdenum steel offers for lighter construction and one relating to manufacturing costs.

Chrome-molybdenum steels show progressively higher physical properties as the molybdenum content is increased up to about 1 per cent. It is claimed that any particular steel in this series will possess, when properly heat-treated, somewhat higher characteristics than other types of alloy steel that are in the same class from a commercial standpoint; or, if steels were sold on the basis of physical properties, the chrome-molybdenum series would offer the greatest values. Tables of test results and many similar data have been published as indicated by the accompanying bibliography,[2] and complete information can be obtained from most producers of alloy steel.

Fig. 1 illustrates the approximate relations of several standard alloy-steels to the chrome-molybdenum series. If it is true that molybdenum steels offer somewhat greater strength, all other things being equal, there should be opportunities for designers to reduce sections and save weight. It is recognized that in many members of a motor car extreme rigidity is required and that in such cases sections cannot be cut down by the employment of stronger material. It seems that there is a possibility, however, of allowing flexibility in some parts that ordinarily are made rigid. The frame is a case in point. It is usual to rely upon the frame to impart to the body the rigidity necessary for maintaining the proper alignment of doors and the like; but it is possible to incorporate stiffening members in the body itself and by suitable attachment of the body to the frame to allow great flexibility in the frame. This should add to the riding qualities of the car.

Fig. 2 illustrates the two methods. Certain cars have been constructed for years in the principle shown in the lower portion of this illustration. Flexible frames can be made of heat-treated alloy-steel and much lighter than the rigid frames. The question has been raised whether the weight that is saved in the frame does not have to be added to the body to give it rigidity. This has not been found to be the case. The body is not called upon to carry so heavy a load and the distance between supports is much shorter. For trucks, the light-weight flexible-frame should be extremely advantageous, as there is apparently no reason for rigidity in most cases.

ADAPTABILITY TO FRAME CONSTRUCTION

Molybdenum sheet steel, both with and without any chromium content, has proved especially adapted to the construction of frames. It can be rolled easily into plates or sheets free from laminations and having clean and smooth surfaces, can be formed without difficulty and, when heat-treated, affords high elastic limits. Reductions in weight can be effected up to 70 per cent of the carbon-steel rigid frame. Table 1 is a comparison of frame steels.

Heat-treated alloy steel frames will have a much

TABLE 1—COMPARISON OF FRAME STEELS

Type of Steel	Condition	Elastic Limit,[3] lb. per sq. in.	Possible Saving in Weight, per cent
Carbon	Untreated	40,500	0
Carbon	Treated	65,400	38
Straight Molybdenum	Treated	90,000	55
Chrome-Molybdenum (Mo 0.30 to 0.50)	Treated	120,000	67
Chrome-Molybdenum (Mo 0.80 to 1.00)	Treated	157,000	70

[3] The minimum elongation in 2 in. corresponding to the above figures for the elastic limit is 16 per cent. The dimensions of the test-piece are ¼ x ⅝ in.

[1] M.S.A.E.—Vice-president and metallurgist, Climax Molybdenum Co., New York City.

[2] Almo Steels, Crucible Steel Co. of America.
Molybdenum Commercial Steels, Climax Molybdenum Co.
Molybdenum as an Alloying Element in Structural Steels by Dr. G. W. Sargent. See *Transactions of the American Society for Testing Materials*, vol. 20, p. 5.
Molybdenum Steels by Dr. J. A. Mathews. See *Mining and Metallurgy*, February, 1921, p. 39.
A Suggested Method for Determining the Comparative Efficiency of Various Combinations of Alloys in Steel by J. D. Cutter. See *Transactions of American Society for Steel Treating*, vol. 1, p. 188.
Molybdenum Steels by Charles McKnight, Jr. See *Transactions of American Society for Steel Treating*, vol. 1, p. 288.
Molybdenum Steel and Its Application by M. H. Schmid. See *Transactions of American Society for Steel Treating*, vol 1, p. 500.
Physical Properties of Materials, Bureau of Standards Circular No. 101.
Molybdenum Frames Mean More Mileage by R. F. Dyer. See *Automobile Topics*, Jan. 22, 1921, p. 1143.

greater longevity and resistance to fatigue, as the Stanton tests of Table 2, which is included through the courtesy of John Miller, metallurgist of the Pierce-Arrow Motor Car Co., Buffalo, indicate. This test approximates service conditions better than any other laboratory life-test of which I know.

TABLE 2—STANTON TESTS OF HEAT-TREATED ALLOY-STEEL FRAMES

Type of Steel[4]	Condition	Number of Impacts Before Fracture
Carbon	Untreated	67 (bent)
Carbon	Treated	3,376
Chrome-Nickel	Treated	11,418
Chrome-Molybdenum	Treated	13,071

[4] Specimens ½ in. wide and 5/32 in. thick; not rotated, supported on edge. Distance between supports. 4½ in. V-notch in center of bottom edge, 0.05 in. deep. Radius at point of notch, 0.02 in. Load applied at center of upper edge, 5½ lb. Weight dropped a distance of 1 in., 100 times per min.

Use for Other Pressed-Steel Parts

The performance of molybdenum sheet steel when used in frames has led to an investigation of its value as applied to other pressed-steel parts such as rims, disc wheels, hubs, rear-axle housings and the like. It is perhaps too early to make a positive statement as to the degree of success that will attend efforts to save weight in such members. The feasibility of heat-treatment has not been proved in all cases, and there may be mechanical limitations that will affect the reduction that would be possible from the purely theoretical standpoint of the strength of materials. The evidence to date, however, leads to the belief that important developments in this field are at hand. It should be noted that the parts mentioned are classed as unsprung weight and whatever saving is accomplished is double important for that reason.

The type of steel that has been most widely used in this connection is an ordinary deep-drawing stock to which about ¼ per cent of molybdenum has been added. Such a steel possesses extreme ductility in the rolled state and considerable strength when heat-treated, as will be seen by referring to Table 3. The test-piece was 1½ in. wide and 9 in. long.

TABLE 3—DUCTILITY AND STRENGTH; ¼ PER CENT MOLYBDENUM CONTENT

Thickness of Sheet, in.	Condition	Elastic Limit, lb. per sq. in.	Elongation in 2 In., per cent
5/32	As rolled	45,640	44.5
5/32	Quenched in water from 1600 deg. fahr. Drawn to 900 deg. fahr.	120,470	7.5

It has been said often that the total amount of alloy steel in the average car is not more than one-fifth of the total weight and that, for this reason, the employment of a steel of slightly greater strength could not affect the total weight by a very wide margin. However, molybdenum steel has without doubt broadened the sphere of usefulness of alloy steels greatly, thus multiplying the opportunities for lighter construction.

Molybdenum Steel and Manufacturing Costs

Molybdenum steel offers the greatest economy in the cost of material for given physical properties in the finished piece. Aside from this first cost there are those of forging, heat-treating and machining. So much has been published on the performance of molybdenum steels in these three basic operations, that only the briefest outline of the characteristics that have been noted will be offered here, together with supporting quotations from technical papers or written reports at various plants.

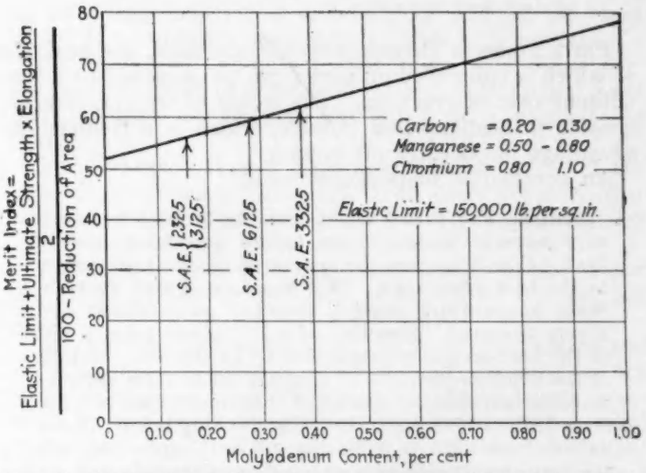

Merit Index =
Elastic Limit + Ultimate Strength x Elongation
2
100 − Reduction of Area
Carbon = 0.20 − 0.30
Manganese = 0.50 − 0.80
Chromium = 0.80 1.10
Elastic Limit = 150,000 lb. per sq. in.
S.A.E. 2325 3125
S.A.E. 6125
S.A.E. 3325
Molybdenum Content, per cent

FIG. 1—APPROXIMATE RELATIONS OF SEVERAL STANDARD ALLOY-STEELS TO THE CHROME-MOLYBDENUM SERIES

In forging, molybdenum steel offers a wide range of permissible temperatures, flows readily in the dies and throws the scale easily. The last quality is most pronounced in comparison with nickel-bearing steels and is a large factor in eliminating rejections.

A forging inspector states,

> It is apparent that chrome-molybdenum steel has a wider forging-range than chrome-nickel steel. We had one of the oldest and best hammer men in the shop on this job and he was highly pleased with the way that this steel worked. A new set of dies was used and the axle was filled out perfectly at all points, which is very unusual with chrome-nickel steel under the same conditions. There was no scrap from the 100 axles forged.

The research department of a large steel company reports,

> It is noticeable that the nickel-steel scale adhered very strongly to the bar and was very difficult to remove; and that the nickel-chrome steel showed the same quality to a less extent. But the scale on the chrome-molybdenum steel came off readily. The effect that this adhesion of scale has on the surface condition of finished steel during the working process is to increase the prev-

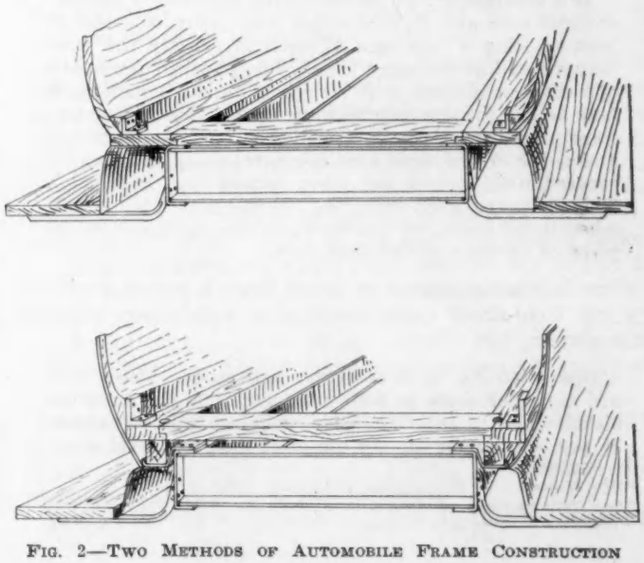


FIG. 2—TWO METHODS OF AUTOMOBILE FRAME CONSTRUCTION

alence of pits and other surface roughnesses on account of having the scale rolled in.

Parts made of chrome-molybdenum steel, the analysis of which is controlled properly, can be quenched in water without fear of cracking. The range of temperature allowable in heat-treating is wide, which is a tremendous advantage in quantity production.

An automobile metallurgist says,

> Chrome and nickel steels have to be held within a very narrow heating-range before quenching and in drawing, to bring out the necessary physical properties to the best advantage. This has necessitated an elaborate temperature control, constant watchfulness and a very thorough inspection of all forgings passing out of the heat-treatment department. In the heat-treating of the experimental lots of forgings made from chrome-molybdenum steel, we quenched at temperatures varying from 1525 to 1600 deg. fahr. Our drawing temperatures varied from 1025 to 1150 deg. fahr. Despite this, all the forgings passed our physical requirements and no trouble was experienced with cracking.

The marked machinability of the steel, especially in the heat-treated state, is the quality that will have the greatest bearing on the decreased cost of production.

Dr. John A. Mathews, in a paper on Molybdenum Steels,[5] that was presented at the 1921 annual meeting of the American Institute of Mining and Metallurgical Engineers, says

> Molybdenum steels seem to machine more readily than other steels at a given Brinell hardness. This would mean, of course, that parts that must be heat-treated and machined afterward could be utilized in a state showing higher physical properties than have been obtainable customarily.

The automobile metallurgist previously quoted on the wide temperature-range possible for quenching and drawing says also,

> On the lathe operation our average production of nickel-steel steering-knuckles, with a Brinell hardness range of 253 to 283 and without regrinding the tools, has been 30. We were able to put through 100 chrome-molybdenum steering-knuckles with a higher Brinell hardness of 311 to 321 without regrinding, an increase of over 300 per cent.

Charles McKnight, Jr., works manager of the Carbon Steel Co., states,[6]

> In a laboratory test the machining qualities of chrome-molybdenum and nickel-chrome steel were obtained by heat-treating a specimen of each in exactly the same manner and at the same time. The physical properties of both were similar, with a slightly higher Brinell hardness on the chrome-molybdenum steel. These specimens were placed in a lathe and while the feed, shape of tool and depth of cut were kept constant, the speed was increased until one or the other turned to a blue color. This occurred first with the chrome-nickel steel and, when it did occur, no discoloration was apparent on the piece of chrome-molybdenum steel.

The following extract is taken from a report prepared by the time-study department of a well-known automobile plant:

> Operation No. 70 is to center both ends. The standard time per piece is 0.30 min. and it is the same on nickel steel. Centering tools will last approximately 25 per cent longer on this material than on nickel steel, according to our check. Operation No. 90 is to drill one hole ¾ in. in diameter, and 1 7/16 in. deep. The standard time per piece is 0.72 min. and it is 0.79 min. on nickel steel. The Baker drilling machine runs at a speed of 250 r.p.m., with a 0.013-in. feed; and at 250 r.p.m. with a 0.012-in. feed on nickel steel. A two-flute Detroit twist drill is used. On the test, 50 pieces were drilled on one grinding of the drill, which was still in good condition. On 3½-per cent nickel-steel the operator has to grind the drills every 10 or 12 pieces; also, there is a breakage of from one to three drills per day's run.

There is another important way in which molybdenum steels may have an effect on manufacturing costs. It is necessary to employ in automobile construction steels of a wide range of physical properties; however, standardization not only in production methods but in materials is the basis on which economical quantity-production exists. The fewer the kinds of material necessary, the greater the efficiency in purchasing and inspection will be. Molybdenum steel offers the possibility of a single basic type, in a few grades differing chiefly as to carbon content, covering all of the numerous combinations of properties required from the extremely hard ball-bearings through the various tempered, toughened or case-hardened articles to the ductility required in deep-drawing stock for pressed steel parts. Further, the analysis range of the several grades may often be contiguous. The benefits and economies accruing to both steel consumer and producer from such a procedure are obvious. The former can employ relatively narrower specification limits, while to the producer the chance of loss from "off-heats" is minimized.

The attempt has been made in this paper to show how the use of molybdenum as the principal alloying element in our steels might be an important factor in the ability of builders to produce lighter and more economical cars which, in turn, should have a favorable effect on the economic status of the industry. It has been stated also that this steel made economies in manufacture possible. It would not be proper to dismiss this subject without a few words as to the available supply of this metal. The huge deposits of molybdenum-bearing ores located in the United States constitute one of the most important and unique natural resources of this Country. The steel industry is assured of an adequate supply to meet any conceivable demand for an indefinite period. This is the only steel-alloying element that is so found within our borders; all of the others must be largely imported. Molybdenum alone can render us independent of foreign sources of supply for the metals necessary to the production of alloy steels.

THE DISCUSSION

A MEMBER:—Do you ever advise the use of molybdenum steel in the non-heat-treated condition for pressed-steel parts?

JOHN D. CUTTER:—The molybdenum steel that has been used in frames and in other pressed-steel parts exhibits a somewhat higher elastic limit in the rolled condition than the ordinary soft steel; an advantage can be gained but it is not nearly so important a one as when heat-treatment is employed.

A MEMBER:—Is the advantage as high as 10 or 15 per cent, or higher?

MR. CUTTER:—I think it is perhaps somewhat higher than that; about 20 per cent might be gained, with a 20-per cent reduction in weight.

A MEMBER:—What of the increased cost of heat-treatment and of straightening after heat-treatment?

[5] See *Mining and Metallurgy*, February, 1921, p. 39.

[6] See *Transactions of American Society for Steel Treating*, vol. 1, p. 500.

MR. CUTTER:—Some frame manufacturers have figured that the cost of the frame made of heat-treated alloy-steel is no greater than that of the soft-steel frame not heat-treated. In other words, the reduction in weight effected offsets the increased cost of the material and the cost of heat-treatment.

A MEMBER:—Does that apply to reduced sections?

MR. CUTTER:—Yes.

A MEMBER:—How does that affect the stiffness when the sections are reduced?

MR. CUTTER:—The stiffness is reduced when the sections are lightened.

A MEMBER:—Then it is not advisable to reduce the sections.

MR. CUTTER:—If stiffness is required, it cannot be done. For passenger cars, unless a change in body design similar to that illustrated in my paper is effected and the frame is not required to be rigid, there is no advantage in the heat-treated alloy-steel frame except that it overcomes the possibility of permanent set and has a higher factor of safety.

A MEMBER:—With the lighter section of steel a frame is produced that is flexible. Will this not allow greater wear on the spring-shackles and bearings?

MR. CUTTER:—I can conceive of that, yes. There have been several cars and trucks that have employed flexible frames for years and there has been no trouble from that source.

A MEMBER:—Is the finish any better than with carbon, chrome-vanadium or chrome-nickel steels?

MR. CUTTER:—I think no pits are eliminated as compared with straight carbon-steel; the finish has been reported to be much superior to that of nickel-bearing steels and of chrome-nickel steels also.

A MEMBER:—Is molybdenum steel rust-proof?

MR. CUTTER:—There is a patent on a very soft low-carbon steel that contains a small amount of molybdenum and is supposed to be rust-proof, but I have never proved that myself.

S. P. HESS:—Is this type of steel suitable for leaf springs?

MR. CUTTER:—It is a very good spring steel. The Fifth Avenue Coach Co., of New York City, had something like 60 such springs on its omnibuses and has been keeping an accurate record, taking them all off, measuring each set periodically and comparing them with the other alloy-steel springs that they use. Their report is very favorable. Only three springs have been broken to date, which is a lower percentage than they have ever attained before. It may be 10 years before they will present a full report.

MR. HESS:—What is the elastic limit in comparison with that of chrome-vanadium steel?

MR. CUTTER:—It is approximately the same; generally, it is about 180,000 lb. per sq. in. for most springs.

MR. HESS:—What is the comparison as regards length of life?

MR. CUTTER:—I think the bus test which I mentioned is the best one that I can quote, as indicating the length of life. My opinion is that fatigue is more dependent on the homogeneity of the steel than on any given analysis. No matter what their composition, I believe that if steels were subjected to identical physical stresses in a static test and were identically homogeneous, there would be very little difference in the length of life.

MR. HESS:—What should be the percentage of molybdenum content of spring steels?

MR. CUTTER:—The molybdendum content in the steels actually used has varied between 0.3 and 0.5 per cent but it can run from nothing to as high as 1 per cent.

MR. HESS:—I understand that there is a slight percentage of copper in molybdenum steels. Does that have any influence on molybdenum steels in a heat-treated state?

MR. CUTTER:—It may have an influence. Most of the ores of molybdenum are very free from copper. Our own molybdenum ore is almost entirely free from any copper-content.

MR. HESS:—Is not the ore found usually in the copper districts? Possibly the trace of copper is due to that fact.

MR. CUTTER:—It is found in the Rocky Mountains and so is copper, but there is not very much copper near the deposits. There is merely a trace discoverable occasionally and most concentrates that are on the market today are very free from copper. There is no danger of copper being introduced into molybdenum steels as a class or of its having an injurious effect on the steel.

MR. HESS:—What is the approximate limit of heat-treatment on molybdenum steel for springs, with reference to the quenching operation?

MR. CUTTER:—When the carbon content is as high as it is in spring steel, I believe that the range is nowhere near as wide as it is in the lower-carbon forging steel. Springs should be heat-treated accurately.

MR. HESS:—I ask that question because we know that chrome-vanadium steel has a wide range.

MR. CUTTER:—I believe chrome-molybdenum steel has an equal range, but that fatigue or impact tests are apt to narrow that range in both steels.

H. G. PEEBLES:—The Bureau of Mines states that the penalty is high for copper content in the ores. Has any headway been made in the last 2 years regarding the separation of copper from the molybdenum in the ores of the Southwest? I understand that the Mexican mines have been closed on account of the copper content.[7]

MR. CUTTER:—We had some concentrates offered us from that district not long ago and rejected them on account of the copper content. There are some deposits that have a large copper content but ours have not. The Mexican molybdenum mines are negligible.

C. W. JINNETTE:—Is molybdenum steel used in crankshafts, camshafts and piston-pins?

MR. CUTTER:—Yes, it has been eminently approved in a number of places. Some shops have found that they can eliminate straightening operations by the use of molybdenum steel in crankshafts because the distortion is not so great; the machinability is good. The piston-pins and camshafts would be of carbonizing steel, and it appears that the elimination of chromium in the carbonizing steel would make even a better steel than the chrome-molybdenum case-hardening steel. There is a very pronounced effect of the molybdenum in steel for carbonizing purposes. The carbon penetrates very much more rapidly than it does with a steel containing nickel. In some cases the time has been reduced materially in the carbonizing process. The core can be made extremely tough and the case hard and strong.

MR. JINNETTE:—Is there any trouble due to cracking?

MR. CUTTER:—No; I have never heard of any such trouble.

MR. JINNETTE:—How do the tooling or grinding costs compare?

MR. CUTTER:—I have no such data. I presume that would be a function of the hardness.

MR. JINNETTE:—We find in grinding that we have had

[7] See Bureau of Mines Circular No. 3, p. 37.

slightly more trouble and that we require softer wheels. The metal takes a beautiful finish.

MR. CUTTER:—But do you not find that the wheels must be changed oftener and that you cannot use the wheels for so long a time?

MR. JINNETTE:—Yes, it requires wheels from 1 to 3 grades softer and in some instances a little coarser than the ordinary wheel used on camshafts and crankshafts made of drop-forged low-carbon steel. The grades of wheel ordinarily used are too hard on both the finishing and roughing operations.

A MEMBER:—Have you rolled any steel as thin as 0.035 in. and, if so, have you had any difficulty in rolling it?

MR. CUTTER:—No, the thinnest stock has been 0.064 in.

A MEMBER:—In rolling a sheet like that without lamination, have you had any difficulty in keeping it flat?

MR. CUTTER:—No such difficulty has been reported by anyone who has done that work. I am having some steel cold-rolled to 0.01 in.

R. W. WILSON:—Have you made any recommendation regarding molybdenum steel for axle shafts?

MR. CUTTER:—The specification would depend upon the design and the stress to which the axle shaft would be subjected. The type of molybdenum steel that has been most widely used and recommended for axle shafts is of 0.40 per cent carbon, with 0.80 to 1.10 per cent of chromium and 0.25 to 0.40 per cent of molybdenum.

MR. WILSON:—What elastic limit does it have in the heat-treated condition?

MR. CUTTER:—That depends upon the drawing temperature. Elastic limits can be obtained for axle shafts as high as 175,000 lb. per sq. in., and perhaps higher than that.

MR. WILSON:—What elongation would that permit?

MR. CUTTER:—About 12 per cent.

MR. WILSON:—How does that steel compare with the S.A.E. No. 3140 steel in price?

MR. CUTTER:—The type of steel I mentioned would be somewhat more expensive than the No. 3140 steel. It would cost about the same as 3½-per cent nickel or S.A.E. No. 2340 steel.

W. C. PETERSON:—In connection with gears and particularly transmission gears that are oil-hardened, what are the abrasive qualities of molybdenum steel?

MR. CUTTER:—Very few oil-hardened gears have been made of chrome-molybdenum steel. Transmission gears have been made of a chrome-nickel-molybdenum steel that, so far as I know, have exhibited the best qualities in regard to resistance to abrasion. They are used in the Wills-St. Claire car. The steel showed an elastic limit of about 300,000 lb. per sq. in.

MR. PETERSON:—Is it true that the elongation curve runs in a straight line over a considerable part of the drawing temperature-range? I refer to the chrome-molybdenum series of 0.40 per cent molybdenum, 1 per cent chromium and about 0.40 to 0.45 per cent carbon. Does not the elongation curve remain a straight line over a distance say from about 500 to 800 deg.? If that be true, does the impact change from a comparatively low value at 500 deg. to a considerably higher value at 800 deg.?

MR. CUTTER:—It is considerably higher, undoubtedly, at 800 than at 500 deg.

MR. PETERSON:—But the elongation does not change; that is different from other steels.

MR. CUTTER:—I think the range you state is somewhat high. I think the elongation curve is rather flat at low drawing-temperatures; that is, if one goes back from 200 to 600 deg., but it surely begins to rise before 600 deg.

MR. PETERSON:—The curve remains a straight line from 400 to 750 deg. and the impact value at 400 deg. was low. It was out of order as compared to other steels. I had time only to find out that the impact value was gradually becoming greater as we came near 750 deg.

MR. CUTTER:—The tests that I have from steel companies on that type of steel do not show that pronounced flatness of the elongation curve at such high drawing-temperatures as you mention. I have no impact-test result on that type of steel at those low drawing-temperatures.

A MEMBER:—Has molybdenum any advantages over tungsten, as an alloy in cutting tools? The experiments carried out by the Navy Department some 5 or 6 years ago indicated that it was inferior to tungsten for roughing work and, in a small proportion, it seemed to help out the cobalt-chrome combination, but the matter was dropped after a very short time and, so far as I know, no work has been done in that direction for a number of years past. As I understand it, molybdenum acts as a substitute either for tungsten in a cutting tool or for cobalt in a service member, so to speak. Just as a rough approximation, how would you compare molybdenum to tungsten?

MR. CUTTER:—I would not compare it because, in high-speed or cutting steels where molybdenum is used as a substitute for tungsten, a content of 4 to 8 per cent is required and, when added to steel in those relatively high percentages, molybdenum has been found not practical. Cutting steels made with molybdenum as the principal element have been extremely efficient and given very good results on cutting tests, but it has never been possible to duplicate those tests; it has never been possible to make a number of pieces of steel that would show a high average result. I think the reason is that molybdenum has a tendency to volatilize near the surface of the steel when added in those high percentages. In fact, a yellow smoke can be noticed coming off the steel when it is heated for forging or rolling. Perhaps there is a chance for a molybdenum high-speed steel for tools that are to be ground considerably, but for a standard steel to be used for all purposes I think that it is not practical at all. So far as I know, no molybdenum high-speed steels are being made in this Country. In combination with cobalt, there have been molybdenum high-speed steels for which great things were claimed, but they have been withdrawn also.

A MEMBER:—I presume that the molybdenum chip is rather fragile; that it is torn off in a brittle flake rather than pulled out like a fiber.

MR. CUTTER:—We know that is the case with screw stock. H. T. Chandler, of C. H. Wills & Co., has an interesting explanation of the superior machinability of molybdenum steel in the heat-treated state as compared with other alloys. He attributes it to the greater reduction of area because, as a tool passes through the steel, the steel that has the property of necking down the most, as exhibited in the tensile test, will not have so much material affected on either side of the tool, and therefore it machines more easily.

A MEMBER:—In what way would molybdenum be superior to tungsten in the ordinary service members of the automobile? So long as tungsten sells at about one-half the price of molybdenum, it certainly would be a competitor as regards cost.

(*Concluded on page* 347)

The Summer Meeting

AS this issue of THE JOURNAL goes to press, the number of reservations for the Summer Meeting has reached 421. Consideration of the fact that the meeting will not convene until June 20 convinces one that a large attendance is assured, and also serves to remind the tardy that the choicest rooms are being disposed of rapidly. For the convenience of those who have not yet forwarded their applications for rooms, a blank is printed on page 69 of the advertising section of this issue of THE JOURNAL. Mail this to the offices of the Society with your check or money order at once. The hotel is not permitted to accept applications direct.

THE TECHNICAL SESSIONS

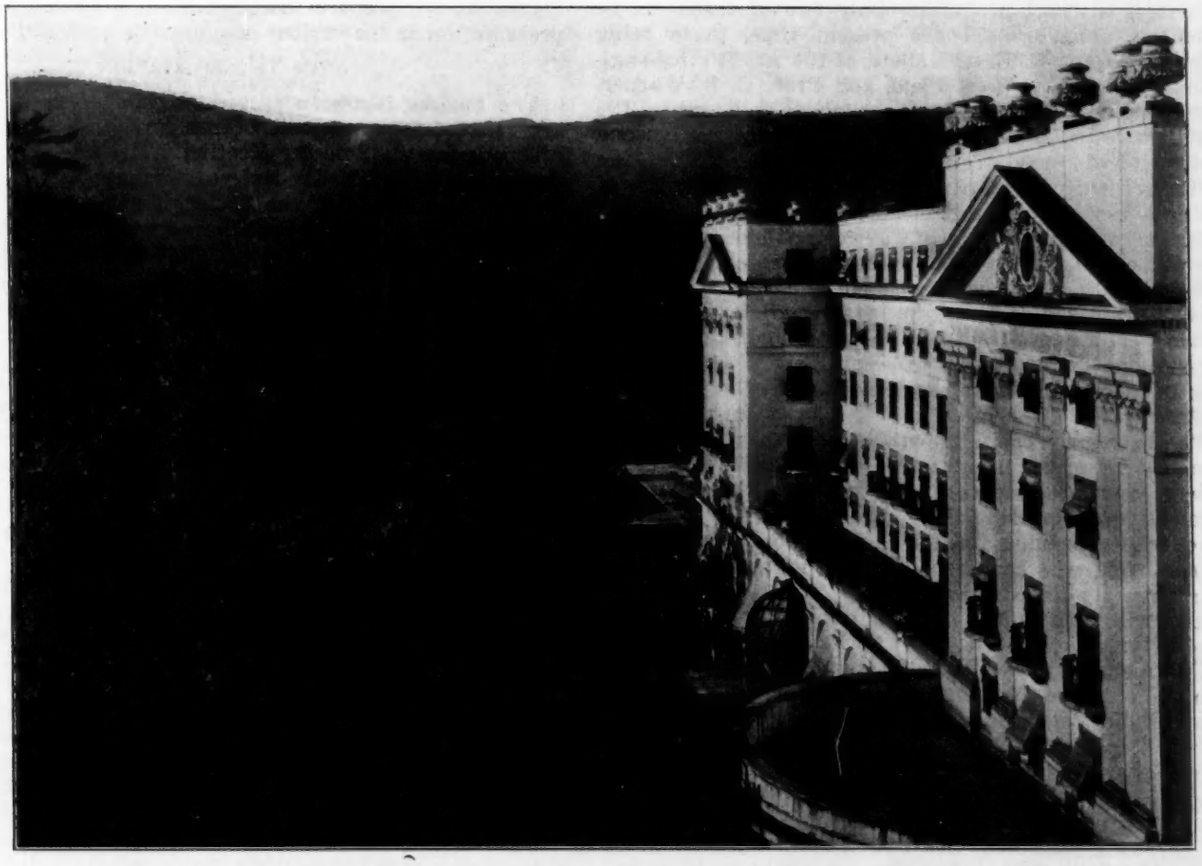
Progress has been made by the Meetings Committee in the selection of papers to be read at the Summer Meeting and the program is practically completed at this early date. Six sessions have been decided upon and will be known as the

(1) Research Session
(2) Fuel and Engine Session
(3) Passenger-Car Session
(4) Aeronautic Session
(5) Motorbus Session
(6) Service Engineering Session

A report from the Research Department of the Society will be the nucleus of the Research Session. This report will treat principally the motor-fuel-volatility and highway-research projects in which the Society has been participating. Fuel tests are being conducted as outlined by the Research Department, the Bureau of Standards and the Bureau of Mines, and the full support of the National Automobile Chamber of Commerce and the American Petroleum Institute is expected.

Several papers will be presented in the Fuel and Engine Session. Thomas Midgley Jr., whose contributions are always received with interest, will submit some data on the characteristics of blended fuels. Two papers will be contributed on the pumping of oil in engine cylinders, a current problem demanding special study. One of these will be presented by an engine designer, A. A. Bull, and the other by a lubrication engineer, G. A. Round. A paper on the present status of the hot-spot method of handling fuels of low volatility will complete the group of papers in this session. This will be presented by F. C. Mock.

Complete plans for the Passenger-Car Session are not ready for announcement, although two papers have been definitely settled. Our national meetings would not seem complete without at least one technical paper from the automotive laboratory of the Bureau of Standards. This summer its paper will relate to a device perfected by the Bureau that records graphically the fuel consumption of a passenger car while it is being operated under ordinary driving conditions. An analysis of records kept over a long period reveal some rather unusual facts



LOOKING WEST ALONG THE SOUTH FACADE OF THE GREENBRIER TOWARD THE MOUNTAINS IN THE VICINITY OF WHITE SULPHUR SPRINGS

RAILROAD ACCOMMODATIONS TO WHITE SULPHUR SPRINGS, W. VA., FOR SUMMER MEETING
JUNE 20-24, 1922

		TIME SCHEDULE				RAILROAD FARES	
City	Railroad	Leave	Arrive Cincinnati	S. A. E. Special Leaves Cincinnati	Arrive at White Sulphur Springs	Fare-and-Half Round-Trip	Tourist Round-Trip
Cincinnati	Chesap'ke & Ohio			9:45 p.m.	8:55 a.m.	$19.08	$20.62
Detroit	Big Four	11:45 p.m.	8:55 p.m.	9:45 p.m.	8:55 a.m.	28.08	31.97
Toledo	Big Four	1:35 p.m.	8:55 p.m.	9:45 p.m.	8:55 a.m.	24.98	28.22
Chicago	Big Four	1:00 p.m.	9:00 p.m.	9:45 p.m.	8:55 a.m.	34.47	38.70
Indianapolis	Big Four	6:15 p.m.	9:00 p.m.	9:45 p.m.	8:55 a.m.	25.01	27.75
Cleveland	Big Four	3:00 p.m.	9:35 p.m.	9:45 p.m.	8:55 a.m.	29.01	31.11
Dayton	Big Four	7:57 p.m.	9:35 p.m.	9:45 p.m.	8:55 a.m.	22.02	22.73

City	Railroad	Leave	Arrive Washington	Leave Washington	Arrive at White Sulphur Springs	Fare-and-Half Round-Trip	Tourist Round-Trip
Boston	New Haven	9:00 a.m.	8:45 p.m.	10:15 p.m.	7:05 a.m.	$37.76	$47.24
Providence	New Haven	10:06 a.m.	8:45 p.m.	10:15 p.m.	7:05 a.m.	36.74	45.88
New Haven	New Haven	12:54 p.m.	8:45 p.m.	10:15 p.m.	7:05 a.m.	29.28	35.94
New York City	Pennsylvania	3:40 p.m.	9:25 p.m.	10:15 p.m.	7:05 a.m.	25.37	30.92
Manhattan Transf	Pennsylvania	3:58 p.m.	9:25 p.m.	10:15 p.m.	7:05 a.m.	25.37	30.92
West Philadelphia	Pennsylvania	5:56 p.m.	9:25 p.m.	10:15 p.m.	7:05 a.m.	20.51	24.89
Baltimore	Pennsylvania	8:30 p.m.	9:25 p.m.	10:15 p.m.	7:05 a.m.	15.32	18.68
Buffalo	Pennsylvania	9:10 a.m.	8:50 p.m.	10:15 p.m.	7:05 a.m.	34.25	39.56
Washington	Chesap'ke & Ohio			10:15 p.m.	7:05 a.m.	13.16	17.28

which command the attention of all who covet an increased number of miles per gallon. W. S. James will present this paper. The wider adoption abroad of overhead-valve passenger-car engines may indicate the tendency of future engine design in this Country. At any rate, a paper on the subject is pertinent at this time and P. M. Heldt will present it in this Session.

The Aeronautic Session will include papers on several phases of aircraft progress. Only two of these can be definitely announced at the present time, these being offered by Capt. G. E. A. Hallett, of the Air Service engineering staff at McCook Field, and Prof. E. P. Warner. Capt. Hallett will describe the methods followed by the Air Service in the design, experimental construction and testing of its new engine types. The success of the McCook Field organization in the rapid development of radically new engines may mean that some of its methods will apply advantageously in the work of industrial engineering organizations. The paper by Professor Warner will deal with a method of estimating airplane performance. It is based on theory but practical data taken from actual performance-tests will be presented. The Meetings Committee also hopes to secure papers on the progress in metal construction and the application of duralumin.

The extremely rapid growth of motorbus transportation has naturally brought this branch of the industry into greater prominence and interest in this phase of automotive engineering is at a high pitch. Two papers have been scheduled for the special Motorbus Session. G. A. Green, who has been identified with the motorbus ever since its introduction in New York City and elsewhere, will treat of the important factors that influence bus design and construction. R. E. Plimpton will submit data showing the unusual growth of bus transportation and compare the many types of bus chassis now produced, indicating the particular service to which each is suited.

Improvement of repair and maintenance service, decreasing repair costs and the engineer's part in advancing these steps will hold the attention of the members at the Service Engineering Session. Two major papers will be read and a number of shorter ones will be discussed in the form of a symposium. J. B. Bray will offer the constructive criticisms of the factory service-manager. His paper will not deal so much with design as with practices of the average engineering department that are detrimental to the execution of an ideal service policy. The engineer's views on the same subject will be set forth in the other paper by one of our members well qualified to present them.

Many of the papers will be circulated in preprint form or included in the June JOURNAL, so that all may have ample time to prepare thorough discussions of them for presentation at the various sessions.

RAILROAD TRANSPORTATION

The Society has been exceedingly fortunate in securing reduced-fare concessions from practically all of the railroads for the Summer Meeting. The plan will follow that used at West Baden last year and the privileges are limited to members of the Society and *dependent* members of their families. Certificates will be mailed to all members about June 10 that entitle the purchaser to a fare-and-half rate for a round-trip ticket. The certificate must be presented when your ticket is purchased and requires validation at White Sulphur Springs before boarding the return train. All tickets purchased under this plan can be used on the S.A.E. Special trains announced herein. The secretaries of the Sections will be provided with an extra supply of certificates for those who may need them.

Two S.A.E. Special trains have been arranged for to transport the Eastern and Western contingents to the Summer Meeting. The use of these trains by the members has much to recommend it; there will be no aggravating changes to make; you will travel with your fellow members of the Society and your friends in the industry. A railroad representative will be in charge of each train to see that everything runs smoothly. Consult your Section Secretary or write the Society offices to make reservations on either train. The Midwest Section will have special Pullmans out of Chicago. Detroit and Cleveland will also provide special Pullmans. Metropolitan, New England, Pennsylvania and Washington Sections will all have special cars on the Eastern train.

The tourist fares are shown in the table for the benefit of those who have non-member guests coming to the meeting. The guests may avail themselves of the lower tourist-rate. The Society is held responsible under the

Interstate Commerce laws for any violation of the agreement limiting the fare-and-half return privilege to members and dependent members of their families. *No tickets will be validated at White Sulphur Springs for non-member guests.*

SPORTS AND ENTERTAINMENT

The next issue of the *Meetings Bulletin* will include the detailed sports program of the Meeting and an entry blank for those who plan to engage in one or more of the numerous events. The companies in the industry have contributed liberally to the prize-purchase fund and the Sports Committee can assure the members appropriate and excellent rewards for the victorious. Tennis, golf, swimming, track and field events will be arranged for the men. Croquet, golf, quoits and cards will test the skill of the ladies. There will be inter-Sectional baseball games, relay races and tug-of-war. Every imaginable branch of sport is provided for at White Sulphur Springs. The tennis courts are first class; the golf course is one of the finest in the South; the natatorium is modern and attractive. You are certain to enjoy your visit to the 1922 Summer Meeting.

The Entertainment Committee is centering its efforts on the many diversions that are being planned for the ladies who attend the White Sulphur Springs Meeting. Several novel departures are to be featured; and the evening dances and movies of past meetings will be outdone. The ladies will have the option of no idle moments at White Sulphur Springs, rest assured of that.

Viewed from all angles, it is safe to predict that the 1922 Summer Meeting will be unusually attractive and well attended. Spend your vacation with your friends in the Society at beautiful White Sulphur Springs.

Most important of all, send your reservation in at once. The blank is on page 69 of the advertising section.

MOLYBDENUM STEEL IN THE AUTOMOTIVE INDUSTRY

(*Concluded from page* 344)

MR. CUTTER:—Such tests as I have witnessed do not show the combination of strength and ductility that is obtained with molybdenum. I think that the tungsten steel is rather apt to be lacking in ductility.

A MEMBER:—So far as I know, no one has ever seriously advocated tungsten steel for automobile parts. That is very likely due to the past high price of tungsten and the demand for that element in cutting tools.

MR. PETERSON:—To what members in an automobile, aside from the valves, do you refer to?

A MEMBER:—I presume that it would be practicable to use tungsten steel in any member in which increased strength would be desirable. Considering the general properties of tungsten as compared with molybdenum, it seems that it would be reasonable to experiment with it in that direction.

MR. PETERSON:—I believe that considerable experimenting has been done along those lines. So far as I have learned, metallurgists are satisfied that tungsten can function only in the cutting tools because of its ability to give the red-hardness property, but other than that it has no value.

MR. CUTTER:—I believe that even if tungsten should show some degree of usefulness for structural steel, the supply of it would be too limited to make its use possible commercially.

P. T. LARGE:—The ductility of molybdenum steel has commended itself to some of the automobile wheel-rim manufacturers.

MR. CUTTER:—I think that developments in that connection will be interesting. There is undoubtedly a demand for a reduction in unsprung weight, especially in the peripheral weight of the wheel, which is the rim. I believe a great advance is possible, that heat-treated rims are feasible and will be produced very shortly. The section ought to be reduced at least one-half. Such rims have been made for installation on racing cars with very good results.

GEORGE L. BIXBY:—Provided the body were properly supported or stiffened, would you consider it feasible to take 70 per cent of the actual weight out of the frame?

MR. CUTTER:—From the standpoint of strength of materials, yes. There may be limitations that would affect the possible reduction of weight, such as the riveted joints in the frame. As the section is reduced, one must take care of the attachments for step-hangers and the like. The method has been to use a larger number of smaller rivets. We do not know yet how far that can be carried. The 70-per cent reduction in weight was based on a steel of 1/16-in. gage, that has been used in a very light frame that I believe is being run on an experimental car now. So far as I know, it has been maintained successfully. I think one cannot use steel very much thinner than 1/16 in.

A MEMBER:—One example of extreme flexibility in automobile frames is the case of the Orient buckboard. I think it had no springs; the road shock was absorbed entirely by the body of the vehicle itself.

RECENT INTERNAL-COMBUSTION ENGINE RESEARCH

(*Concluded from page* 336)

using the heavy crude fuel that you have been using in a Diesel engine.

Mr. McDewell asked an interesting question as to how far dissociation plays a significant part. I have no actual figures available at the moment, but certainly dissociation plays a very important part in determining the maximum flame-temperature; but that is all. Neither Mr. Tizard nor I have contended that dissociation plays any very serious part in the efficiency of an engine. Probably, and I think it is quite arguable, if one suppresses the dissociation altogether, the efficiency would be lower because of the higher temperatures involved and the consequent direct heat-loss. It does, however, play a part in that it puts the value of high compression on a better footing than it would appear. I think this is the main point, that it gives one a higher efficiency at high compression-ratios than one would otherwise expect to obtain.

Mr. McDewell also inquired whether the good luck I had with the stratified-charge engine applied also to the supercharging engine. Insofar as the stratification of the charge was concerned, I had plenty of trouble and very little luck with this engine in other directions, chiefly due to excessive detonation and, until I eventually discovered that air-scavenging was the cause of this, I could make very little progress.

Paint Protection for Wood

By Cornelius T. Myers[1] *Illustrated with* Charts

ABOUT two years ago in carrying on some research work on wood wheels I had occasion to make inquiry among paint manufacturers as to the value of various kinds of paint and primer for the protection of wood against moisture. This inquiry brought out so many differences and variations in opinion as to paint materials and mixture-proportions that a more extensive inquiry among paint men was started. This inquiry revealed that

(1) There was little or no technical information to be gained from the paint industry on the relative protective values of different coatings for wood

(2) The paint industry did not recognize the moisture-proofing of wood as a problem, but was concentrating so far as the more reputable manufacturers were concerned on producing paints that would last as long as possible on the surface to which they were applied. In other words, such improvements as have been made have contributed to the life of the coating rather than the life and usefulness of the article that received the coat

As the demand upon our forests depends to a very considerable extent upon the life of forest products, and as the life of these products in many cases depends to a very great extent upon the protection against moisture they receive, the intimate knowledge of protective coatings is a factor of great importance in our lumber conservation program; to say nothing of the possible saving in our $300,000,000 annual paint bill. Wood in its natural green condition has a very limited use as a structural material. Its tiny fibers are cellular in form and contain a large amount of moisture, commonly known as sap, the sap being water containing very small percentages of tannins, sugars, gums and coloring matters. To give the physical qualities that make it fit for use in buildings, furniture, truck bodies, wheels, etc., it is necessary to remove most of the moisture.

Moisture in Wood

Wood is said to be oven-dry when continued oven-drying causes no further loss of weight. In their green state the woods used structurally have a moisture-content of from 60 to 120 per cent of their oven-dry weight, and weigh roughly from 60 to 120 per cent more than when they are in an oven-dry state. In other words, from 35 to 55 per cent of the weight of a green log is water. In the green log this water exists in two conditions:

(1) Minute particles of water in each tiny wood cell cavity, known as "free water"

(2) The moisture absorbed by the fibrous material that forms the walls of the cells, known as "hygroscopic moisture"

Felled wood exposed to average atmospheric conditions gradually dries, the air taking up its moisture. First the cell cavities slowly give up their moisture. This evaporation goes on until the cell cavities are emptied of the minute bodies of "free water" they contain. All that remains is the moisture actually absorbed by the fibers of the cell walls, which are still saturated with the "hygroscopic moisture." Wood in this state is said to be at its "fiber-saturation point." Up to this time the wood does not change appreciably in size or in physical characteristics except as to weight, which of course decreases considerably, as it will now contain but from 25 to 30 per cent of moisture instead of from 60 to 120 per cent.

Further drying of the wood is necessary to increase its strength and hardness, increase its durability, enable it to take and hold paint and in general to improve its condition for the purpose intended. As this drying progresses beyond the fiber-saturation point the cell walls give up the moisture they have absorbed and in doing so shrink and harden. Under natural conditions this drying process will continue until the amount of moisture in the wood bears a definite relation to the average humidity condition in the particular locality (See Fig. 1). In the Eastern and North Central States, for instance, the moisture-content of wood will become stabilized somewhere in the neighborhood of 14 per cent for what is known as thoroughly air-dried stock. In the arid Southwestern States it will contain a still lower percentage, while in the former war zone of France the wood will normally contain considerably more than

TABLE 1—APPROXIMATE PERCENTAGE OF INCREASE IN STRENGTH OVER GREEN[2]

	Dried to 14-Per Cent Moisture	Dried to 8-Per Cent Moisture
Bending Strength, Modulus of Rupture	40 to 60	80 to 100
Compression Parallel to the Grain	80 to 90	100 to 150
Compression Perpendicular to the Grain	65 to 75	
Stiffness, Modulus of Elasticity	20 to 30	25 to 35[a]
Hardness	30 to 35	40 to 50[a]
Shearing Strength, Parallel to the Grain	40 to 50	60 to 70[a]

[a] Estimated.

[2] Compiled from data given in Timber, Its Strength, Seasoning and Grading by H. S. Betts.

TABLE 2—SHRINKAGE AND MOISTURE CONTENT OF HARDWOOD[3]

	Specific Gravity of Dry Wood	Shrinkage[7] from Green to Oven-Dry in Terms of Green, per cent Radial	Tangential	Moisture in Green Wood in Terms of Dry Wood, per cent
Ash, White[4]	0.57	4.8	7.0	39
Birch, Yellow[4]	0.55	7.4	8.9	68
Elm, Cork	0.57	4.8	8.1	53
Hickory[5]	0.64	7.2	10.9	60
Maple[6]	0.51	4.2	8.5	63
Oak, Red[6]	0.56	3.9	8.3	83
Oak, White[6]	0.60	5.3	8.8	66
Average	0.57	5.4	8.6	62

[3] See Kiln Drying of Lumber by H. D. Tieman, p. 129.

[4] Average of 2 species.

[5] Average of 9 species.

[6] Average of 3 species.

[7] The average shrinkage along the grain or up and down as the tree grows is only about ½ per cent.

[1] M.S.A.E. Consulting Engineer, Rahway, N. J.

14 per cent of moisture. Of course, the moisture content of wood will vary to a certain extent with seasonal changes of atmospheric humidity, the rate at which the wood will follow these changes depending upon the degree of protection by paint and upon the area of the exposed surface. Thus unpainted wood that is exposed to rain and snow will absorb considerably more than 14 per cent depending upon the dimensions of the piece and the extent of the exposure.

Data secured from the Forest Products Laboratory of the United States Department of Agriculture show us that

(1) Many woods should be dried to about 8-per cent moisture-content to give the best results as to strength, durabiltiy, hardness and finish (see Table 1)
(2) It is also true that if after being dried, and shrinking in the process, wood reabsorbs moisture, it will swell again according to the amount absorbed
(3) The shrinking and swelling along the grain, perpendicular to the annular rings and tangential to the annular rings all differ, and very materially, for a given change in the moisture-content (see Table 2)

From the above mentioned tables it will be seen that it is desirable to have the wood thoroughly dried, and that steps should be taken to keep it so. But, although it is easy to reduce the moisture to 8 per cent in dry kilns, it is difficult to maintain the wood at this point, because of unsuitable protective coatings or processes, and of the lack of knowledge as to the relative ability of various coatings for really stabilizing the moisture content within a small range.

Protectiveness of Paint

While we know that several coats of good paint will give adequate protection for floors, truck bodies, furniture, wheels or other wooden articles, we by no means know what paints give the best protection or what paints will give fairly satisfactory protection for the least money. The test fences, where many different paints were exposed to the same atmospheric conditions, have given some data on the durability of paints, but very few data on the durability of the painted article or its dimensional stability. *In durability and dimensional stabilization we are greatly interested;* in the first for very obvious reasons, in the second because wooden structures, furniture and the like are durable and serviceable and can be made more simply if their various component parts do not expand and contract with changing weather conditions. The cost of drying could be reduced in many cases, if when dried to a certain point the moisture-content could be stabilized. Wood, on account of its very valuable characteristics, could be used in places where metal now seems necessary. A dozen or more prominent companies in the paint industry have been cooperating very commendably in this research work, realizing that the dimensional stabilization of wood by moisture-proofing is a proper function of paint and of world-wide importance. There is, however, much difference of opinion among these manufacturers as to what materials should be used and how they should be mixed and applied. During the war the Forest Products Laboratory investigated the moisture-proofing effects of linseed oil and various paints, varnishes and leaf-metal coatings as applied to airplane propellers and other airplane parts made of wood, but this work has not been broadened because of lack of funds.

To get some comparative data on the paints and primers now on the market, I drew up the following schedule and tests were made on small pieces, measuring

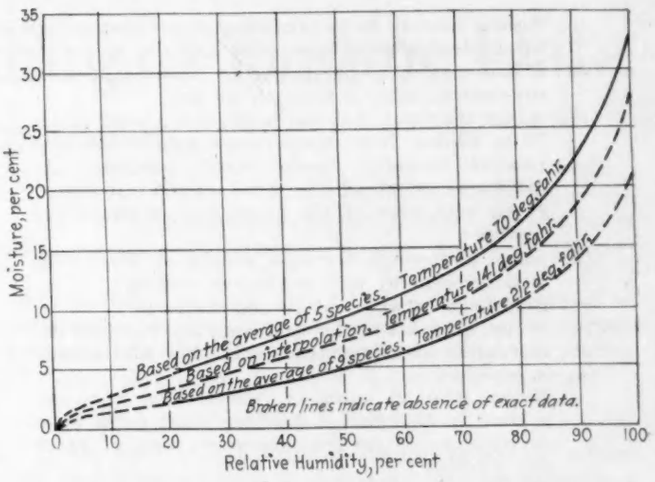

Moisture, per cent
Relative Humidity, per cent
Based on the average of 5 species. Temperature 70 deg. Fahr.
Based on interpolation. Temperature 141 deg. Fahr.
Based on the average of 9 species. Temperature 212 deg. Fahr.
Broken lines indicate absence of exact data.

Fig. 1—Relation between the Amount of Moisture Absorbed by Wood and the Average Relative Humidity

1 x 1 x 4-in., of hickory, oak, birch and maple by several well-known paint firms.

(1) Dry slowly and completely until pieces cease to lose weight
(2) Determine "oven-dry" weight
(3) Allow reabsorption to 8 per cent
(4) First coating to be applied at 8-per cent moisture-content

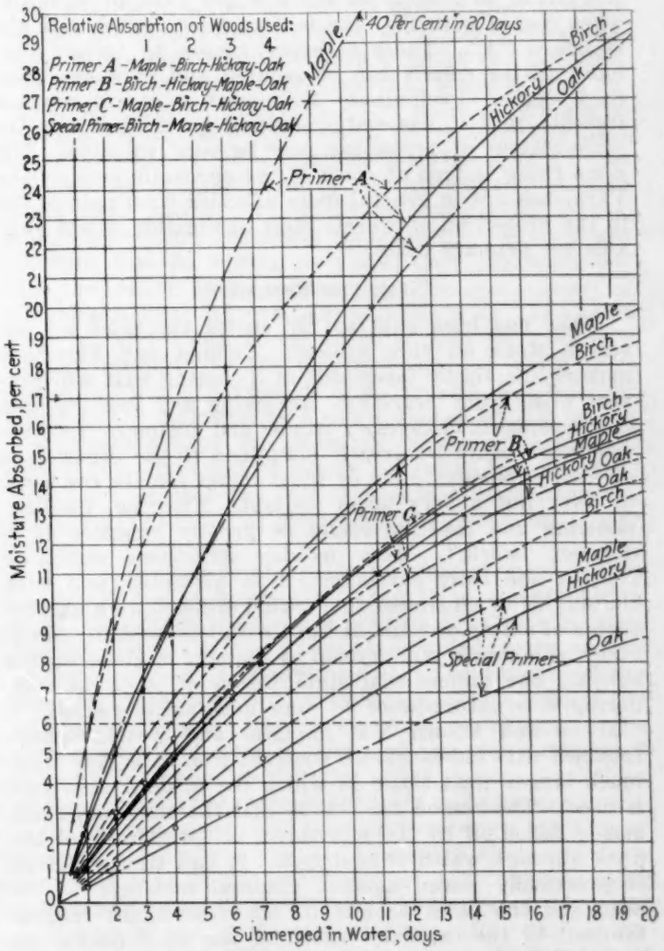

Relative Absorbtion of Woods Used:
1 2 3 4
Primer A - Maple - Birch - Hickory - Oak
Primer B - Birch - Hickory - Maple - Oak
Primer C - Maple - Birch - Hickory - Oak
Special Primer - Birch - Maple - Hickory - Oak
40 Per Cent in 20 Days
Moisture Absorbed, per cent
Submerged in Water, days
Primer A
Primer B
Primer C
Special Primer
Maple
Birch
Hickory
Oak

Fig. 2—Curves Showing How the Effectiveness of Different Primers as Moisture-Proofing Agents Varied for Different Woods

(5) Second coating to be applied at 8-per cent moisture-content after a lapse of 24 hr.
(6) Third coating to be applied at 8-per cent moisture-content after a lapse of 24 hr.
(7) Allow the three coatings to dry for a week under 70 to 80-deg. fahr. temperature and 40-per cent relative humidity, under which condition the moisture-content of the wood should remain at 8 per cent even if the wood had no protective coating
(8) Label and weigh for base weight at 8-per cent moisture-content with protective coating
(9) Submerge in water at 70 deg. fahr.
(10) Weigh every 24 hr. for first week and every 48 hr. thereafter and determine percentage of increase in moisture

Note: Bear in mind, of course, that whatever is used in the way of coatings must serve as a suitable foundation for subsequent coats of paint and varnish

In general, the results of the tests showed that paints of the ordinary brands and formulas are not very effective as moisture-proofing agents, even when three coats are applied. One paint company after testing its standard brands did a little experimenting and without much difficulty was able to produce a special primer that was several times as effective. The curves in Fig. 2 show emphatically the variations in effectiveness of the paints used. Averaging the results for hickory, oak, maple and birch, it took about six times as long for these test-pieces to absorb an extra 6 per cent of moisture when coated with the "special" as when coated with the standard "A". There is every reason to believe that much better results can be secured and with less than three coats. Preliminary tests with a casein solution indicate that it has water-proofing qualities which for some classes of protection may be very valuable. The same thing is true of some of the pyroxalin compounds. Varnishes are in general more effective than paints, but in the protection of wheels their application is not suitable for primary coats.

Need for Research

What has been said so far shows the need for research work on this subject. Cabinet and furniture makers are vitally interested in a coating that will prevent wood from "working," shrinking and swelling, due to atmospheric changes in winter and summer. Interior woodwork could be greatly simplified if the dimensions of each particular piece of wood would remain the same or vary but a very small amount. Flooring, decking, paneling and the like would be greatly benefited if it did not "work." Many wooden structures would be simpler and more permanent if so protected, and with thoroughly dried timber they would either have a greater factor of safety or could be built with less lumber. Many other advantages will develop as thought is given to this subject, the dimensional stabilization of wood by rendering it moisture-proof to some substantial degree.

It is well known that shingles and weatherboards fastened with old-fashioned wrought-iron nails stay tight much longer than those in which the modern steel nail is used. The reason for this is that the steel nail rusts and is flaked off by the movement, or "working", of the piece through which it is driven. A nail tightly driven is practically sealed against external moisture by its head and the paint around it but if moisture reaches the nail by the capillary action of the wood fibers, the nail will rust in spite of end-sealing. Then, too, the moisture and capillarity of the wood cause the "working" that rubs off the layer of rust and exposes the nail to continuous corrosion. The working also abrades the wood and leaves the familiar large, rust-stained hole around an attenuated nail. The same conditions apply to the bolts and other steel fastenings in farm machinery, wagons, motor-truck bodies and a long list of other articles. It is of vast importance in wood construction, therefore, that we have paints to protect the wood from absorbing moisture. Ordinary paint does not do this effectively except when a large number of coats are applied and so far as I have been able to find out, there has been little or no attempt to produce a paint that will do it.

In the realm of house painting there seems to be a great possibility for economies, either by the use of cheaper paints that will give the necessary protection, or by the use of paints that will last longer. I am not familiar enough with this subject to do other than to refer to Dr. A. H. Sabin's letter to the *Engineering News-Record*[5] in which he states that even the Pennsylvania Railroad is not always able to protect itself in the matter of paints, and asks what chance the ordinary citizen has to check up on the paint he should use to protect the house that very possibly represents his life's savings. Paint literature of a technical nature is of no assistance to the average man and is entirely inadequate to explain the whys and wherefores of much present practice. Great differences of opinion and some vague reasoning appear. Many contentions seem to be based on a desire to use certain materials or formulas rather than on comparative service data of a reliable sort. Such men as Dr. Sabin of the National Lead Co., and Dr. Holly of Acme White Lead & Color Works, make frank acknowledgment of the shortcomings of paint technology and totally disavow any detail information concerning the effect of paints in moisture-proofing the cell structure of various kinds of wood. The Engineering Foundation has approved the idea of research in this connection and appointed a committee to report ways and means of furthering it. This is a most substantial endorsement. The American Institute of Architects also will lend its support, and the matter has been favorably considered by the Council of the Society of Automotive Engineers.

It would seem that an intimate knowledge of wood cell structure is essential to any investigation of this subject. The Forest Products Laboratory is by all odds our most authoritative source of information on woods and their structure. The director and his staff have been interviewed and fully agree that this research would be productive of important results, some of which could be expected in the course of 8 to 12 months; also that it can be handled there if funds in a very modest amount, between $10,000 and $20,000 per year, were made available for, say, from 5 to 10 years. The Bureau of Standards has a paint division and has funds to work on the paint end of the research. Doubtless a cooperative arrangement could be made between these two of the most capable and helpful arms of our governmental service. Doubtless, too, the technical heads in the paint and varnish trade would place at the disposal of the Bureau of Standards and the Forest Products Laboratory their great fund of knowledge of paints and oils. Many of these men are already working on this problem as has been set forth above; but they are handicapped by a lack of intimate knowledge of the various kinds of woods, and they are very much limited in the time they can give and the range of materials they can test.

[5] See *Engineering News-Record*, Aug. 18, 1921, p. 291.

Vol. X May, 1922 No. 5

Photographic Recording of Engine Data

By Augustus Trowbridge[1]

Annual Meeting Paper *Illustrated with* Photographs and Diagrams

BELIEVING that it is one of the functions of the purely scientific man to direct engineering attention to practical possibilities that will be of use in solving important problems, the author outlines the history of the photographic recording apparatus he describes later in detail and comments upon its general features that are of advantage in engineering practice, with illustrations, inclusive of the use that is made of the string galvanometer.

The subject of indicators for high-speed engines is discussed in general terms introductory to a full and detailed description of how this automatic photographic recording apparatus can be used to overcome difficulties that pertain to ordinary indicator-diagrams taken on the internal-combustion engine by former methods. A further use of this apparatus is in anti-knock research and its recent usage for this purpose is described and illustrated.

UNTIL a few years ago I shared what I believe to be the general feeling among technical scientific men that photographic methods of recording experimental data such as pressure-volume variations in an engine cylinder, vibrations of shafting and motion or timing of valves, could not be rendered sufficiently simple, cleancut and foolproof to be reliable in the hands of the ordinary intelligent mechanic, but nearly two years' experience in France during the war convinced me that photographic recording is the quickest, cheapest, most cleancut and foolproof method that exists. I was in charge of the sound-ranging service in the American Army, the object of which was to locate the positions of the enemy large-caliber guns by recording the time of the arrival of their sound-waves at known positions near our own trenches. An accuracy of about 50 ft. in 5 miles was required and a delay of more than 4 or 5 min. in recording, calculating and reporting, to our own artillery headquarters was considered excessive. The work was carried on by the ordinary enlisted man who had been given but a few weeks' special training; and it was always conducted under rather unfavorable conditions, in the cellars of ruined houses or in crowded dugouts. I believe that the photographic methods that withstood this test continuously for months without a breakdown are suitable for use under almost any conditions.

Success was due chiefly to the fact that all operations were made strictly automatic. The camera was designed to use a cheap sensitized paper that was fed from a roll past a stationary lens through which the exposure was made. The paper was automatically developed and fixed before it left the camera, so that there was no chance whatever of confusion of records when a great many were being taken in rapid succession, since the operator could inspect and mark with a pencil each record as it issued from the camera.

Since the war, I have adopted this apparatus for the purposes of the physicist when I have needed to make use of photographic methods and, as will be developed somewhat in detail, I think it is adapted to the purposes of those who are interested in securing good internal-combustion-engine performance and in locating the sources of undesirable noises that it is desirable to eliminate.

General Features

In the present apparatus a simple mechanism allows the record to be taken at one speed and developed at a slower speed so as to give sharp contrast. Exposure speeds up to about 4 ft. per sec., with an ordinary automobile electric-light bulb as a source of light, can be used. The automatically developed records show a contrast that is entirely satisfactory. A series of fine lines reaching clear across the paper is photographed at the same time as the record. These lines are spaced 0.01 sec. apart, even if the speed of the paper should vary slightly, and serve as time-lines to time accurately an event shown on the record; for example, the opening of an exhaust-valve.

An automatic recording camera can be used to trace the motion of any moving part of a mechanism and, owing to the high speed at which a record can be taken, it is possible to study variations that occur in time intervals as short as 0.001 sec. It is more convenient to transmit electrically to the camera the motion that actually takes place in the mechanism than to attempt to set up the camera itself close to the mechanism. I have found that a special form of simple string galvanometer is very convenient and meets satisfactorily the necessary condition that its moving part shall move synchronously with the motion of the mechanism. The moving part of the galvanometer is practically weightless, as it is a short piece of wire 0.004 in. in diameter, and its motion is damped strongly by the magnetic field in which it is placed. When a current is sent through this wire, it moves in the magnetic field and a shadow of this motion is photographed by the automatic camera. In practice, almost any slight motion can be converted into a flow of electricity, so that the applications of photographic recording to the study of vibrations in running machinery are very numerous.

If it is desired to study the pressure-changes within the cylinder of an engine that is operating, a capsule with a stiff diaphragm can be screwed into a pet-cock opening and the motion of the diaphragm recorded as the motion of the string of the galvanometer. The records of the pressure-changes made at every stage of the cycle of the running engine are very instructive, as shown in the charts presented later of some of the many records I have made of engines running under various conditions of load, speed, mixture, spark-setting and temperature.

On any one record, it is possible to secure a great many successive explosions and so to compare them and draw conclusions as to the constancy of performance under supposedly constant conditions. The timing of the valves or the spark can be determined *dynamically* with an accuracy of about 0.0005 sec. This means about 6 deg. of crankshaft travel in the case of an engine that is running at 2000 r.p.m. Thus, the lag between the static and the

[1] Professor of physics, Princeton University, Princeton, N. J.

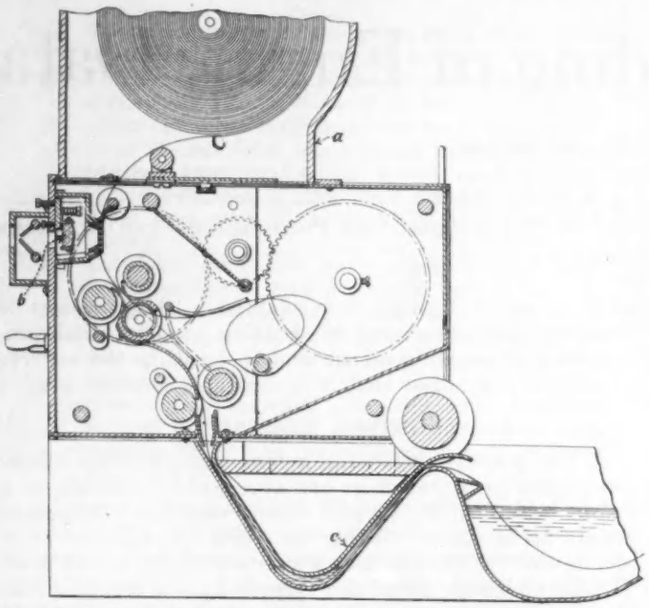


FIG. 1—APPARATUS FOR RECORDING ENGINE DATA PHOTOGRAPHICALLY

dynamic timing can be determined at various engine speeds.

DETAILED DESCRIPTION

The essential feature of a foolproof photographic recorder is that it requires but slight attention from the operator, so that he will make as few mistakes as possible. The portion of the apparatus shown in detail in Fig. 1 is a camera that employs sensitized paper in 200-ft. rolls that are stored in the holder at *a*. This paper is run downward vertically before a lens at *b*. The film is then carried through the developer in the housing at *c* and run out into the fixing bath that lies outside of the camera. After starting to develop a film, one need not fear the effect of light, since the fixing can be carried on in a room lighted sufficiently so that one can see to read easily. The entire apparatus can be operated in daylight. The film is developed in the dark and fixed in daylight; so, the operator can reject films that are not interesting, or he can mark with a pencil those that he wishes to keep.

It is necessary to have some kind of a time-scale on the same paper film. In the device that I have used the beam of light with which the photography is done is cut by the spokes of a wheel that spins accurately, being governed in its spinning by the motion of a tuning fork that is caused to vibrate by electrical means. All this apparatus takes care of itself automatically. The small spinning wheel has spokes that come into the path of the light and cast shadows on the film every 0.01 sec. These shadows are photographed across the film as it runs vertically past the lens and the interval between the photographs of these shadows on the film gives a time-scale of 0.01 sec. This may be as much as 0.50 in. from line to line, if necessary, and 0.50 in. in 0.01 sec. is a very high speed. The light needed can be furnished by an ordinary automobile bulb operated from a 6-volt circuit, such as can be bought at any garage. Fig. 2 shows the spoked wheel at *d* and the tuning fork that drives it, seen in the direction in which the light travels through the lens. Fig. 3 gives a diagrammatic sketch of the electrical connections.

The apparatus as described thus far will record on a rapidly moving strip of paper an accurate time-scale in hundredths of a second and the motion of any moving object or objects such as the shadow of a fine wire that is in motion. There remains to describe how such a fine wire can be made to move so that its motion will be the same as, or related to, the motion of a mechanism that is operating and which it is desired to investigate.

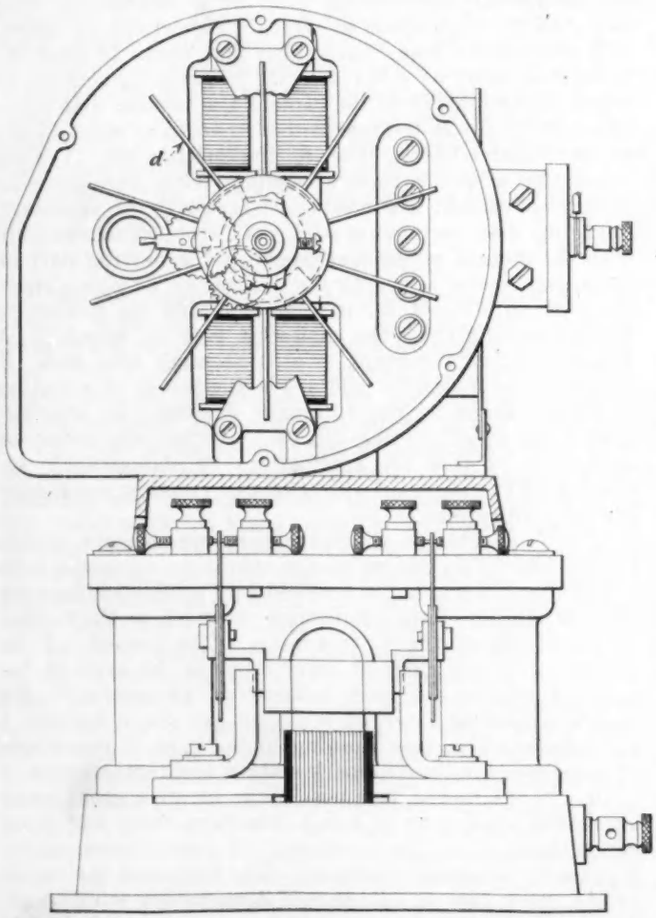


FIG. 2—DETAIL OF THE SPOKED WHEEL THAT CASTS SHADOWS ON THE FILM TO INDICATE THE TIME INTERVALS AND THE TUNING FORK THAT DRIVES IT

THE STRING GALVANOMETER

The device that I have been using is an old one that was discovered by Einthoven, a Dutch physicist, 20 or more years ago. It is known as the string galvanometer and is merely a small electromagnet furnished with current from the same battery that supplies the light, operates the tuning fork and turns the spoked wheel.

In Fig. 4, *N* represents the north pole and *S* the south pole. There is a small hole vertically down through the strongest part of the field. One or more fine tungsten wires, each about 0.0005 in. in diameter and 1 or 2 in. long, run down through this hole as indicated at *e* and *f*. Two lenses are located at *g* and *h*, through which the light from a small nitrogen-filled electric-light bulb passes and causes the fine tungsten wires to cast shadows. When a wire moves, its shadow moves on the photographic film and it is the motion of the shadow that one photographs along with the time-lines that are 0.01 sec. apart.

Fig. 5 shows the complete plan of the optical system. The light is at *i*. A spoke of the wheel, *j*, has come into a horizontal position. The lens at *k* forms a real image of *i*. The rays cross and go through the two lenses *l* and *m*, the light between them being an intense parallel beam. Two small tungsten wires are shown as dots at *n* and *o*,

because they appear only as in a plan view. Their shadows are thrown on the photographic film at *p*. If either wire moves, its shadow moves and one obtains a record of its motion on the photographic film. A blind to regulate the amount of light is shown at *q*, and a cylindrical lens at *r* reduces the images of the wires *n* and *o* to points and the image of *j* to a fine horizontal line.

Fig. 6 shows in elevation the holder of the fine tungsten wires *n* and *o*. Once the wires are mounted in the holder, one is not likely to break them; they are too thin

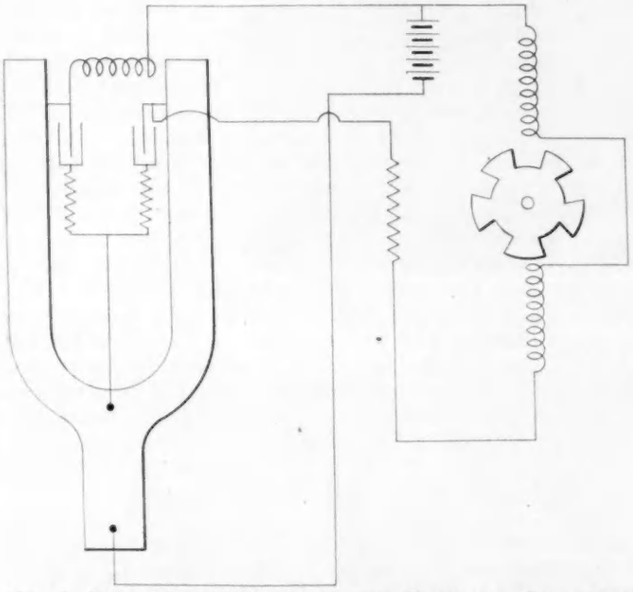


FIG. 3—DIAGRAMMATIC SKETCH OF THE ELECTRICAL CONNECTIONS

to break. If one blows upon them, they offer virtually no resistance. They have practically no weight and, on account of this lightness, they have no free period of vibration of their own and hence reproduce faithfully the motions impressed upon them. To use this device, by which one is able to photograph a tungsten wire 0.0005 in. in diameter that is moving rapidly, it is necessary to cause it to move. Since it is in a magnetic field, it can be caused to move across this magnetic field by sending an electrical current through it. It is merely a case of transforming the motion that one desires to make into a flow of current, sending that current through the

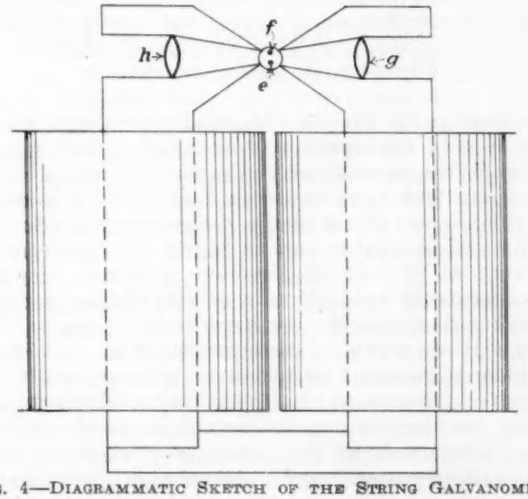


FIG. 4—DIAGRAMMATIC SKETCH OF THE STRING GALVANOMETER

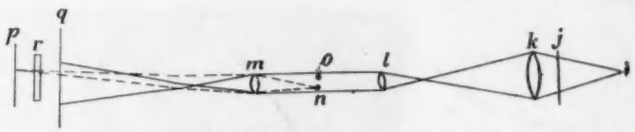


FIG. 5—COMPLETE PLAN OF THE OPTICAL SYSTEM

tungsten wire and photographing the motion of that wire's shadow on the sensitized paper of the camera. I have used this device for a great many purposes and I will now explain how it can be used for obtaining indicator-diagrams from an internal-combustion engine.

INDICATORS FOR HIGH-SPEED ENGINES

There are numerous good and sufficient reasons why the indicator has not played so important a part in the design and testing of the internal-combustion engine as it did in the case of the steam engine. The high speed at which the former is run makes it very difficult to construct an indicator the motion of which is characteristic of the pressure variations in the cylinder and not characteristic of the inertia and elastic properties of the indicator itself. In addition, the standard form of indicator-diagram, in which the pressure of the hot gas in the cylinder is shown as a function of the volume that the gas occupies at any instant, is most unsuitable in the case of the internal-combustion engine, for the diagram is crowded together at the in-center and out-center positions

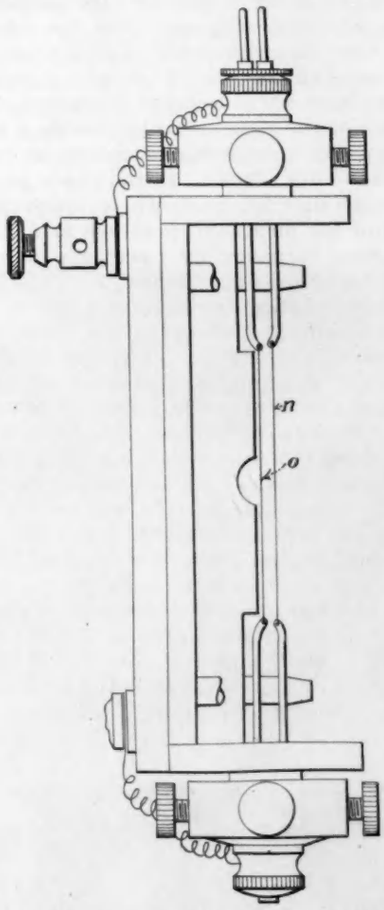


FIG. 6—ELEVATION OF THE HOLDER FOR TUNGSTEN WIRES

and these are just the positions near which the ignition, or spark, occurs and where the valves are opening. Thus, the very portion of the diagram where the most open

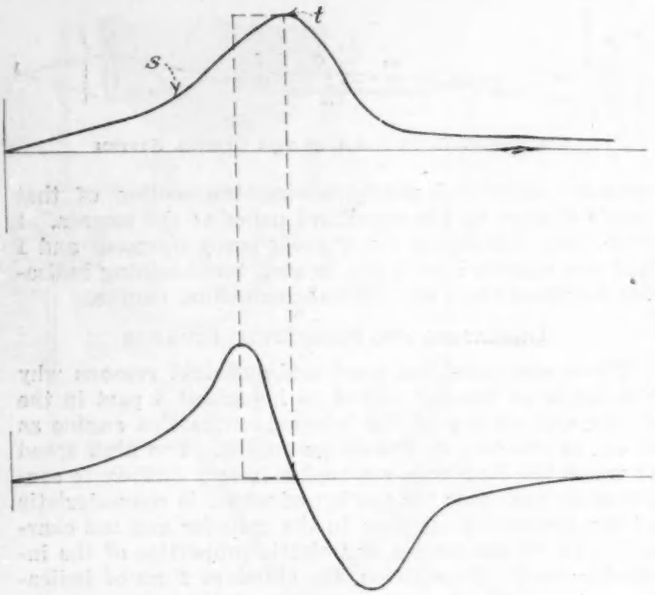


FIG. 7—A PRESSURE-TIME DIAGRAM (ABOVE) AND (BELOW) THE SAME DIAGRAM REPLOTTED IN TERMS OF THE RATE OF VARIATION OF THE PRESSURE

scale is desired is that at which the diagram practically stands still. Engineers acknowledge generally that a more unsatisfactory diagram than the old fashioned pressure-volume diagram would be difficult to devise, and indicators more suitable for work with internal-combustion engines have been designed to record the pressure as a function of the time, so as to provide a scale that is as open near the in-center and out-center positions as at the middle of the stroke. In the upper part of Fig. 7 a pressure-time diagram is shown in which the spark occurs at *s* and the maximum pressure at *t*. This is the type of diagram furnished by a pressure indicator of the type that is being used by Thomas Midgley, Jr.

I have found that a considerable further gain in accuracy is obtainable by changing the form of the indicator-diagram. If, instead of registering the pressure as a function of the time, the *variations of the pressure at each instant of the stroke are recorded*, wherever there is a change of slope on the pressure-time record there will be a nick on the record showing the pressure-variation as a function of the time or phase of the piston motion. In other words, my thought was to record the *rate of change of the pressure with the time*, as being a step farther in advance, in place of recording the pressure with the time or the pressure with the position of the piston. By plotting directly as vertical heights the *rate* at which the pressure is changed with the time instead of the pressure itself, a nick is produced in the curve.

It is naturally much easier to notice and time such a nick than to notice what may be but a slight change of slope on a pressure-time record, and such records are in consequence much easier to interpret with accuracy. After all, the chief use of an indicator is to assist one in noticing *changes* in the running conditions of an engine when *changes* in the fuel, spark-setting, speed and the like are made, and therefore an indicator that is designed to bring out *changes* is the most useful form practically.

It is easy to construct an indicator that will record the rate at which the pressure is varying instead of recording the pressure itself. An indicator in which a very stiff steel diaphragm is the moving part can be made so that the diaphragm moves in step with the pressure exerted upon it, and introduces no vibrations peculiar to itself. If a small bobbin of fine wire be mounted rigidly on the diaphragm so that in its motion it cuts a radial field of magnetic force, an electromotive force is induced that is in step with the *velocity* of the bobbin; that is, with the time-rate of variation of the pressure exerted on the diaphragm that supports the bobbin. The currents that are induced by the movements of the bobbin in that magnetic field are proportional to the velocity of the diaphragm and, as the velocity is proportional to the rate at which the pressure is varying with the time, one obtains an indication of the pressure-variation in the cylinder with the time that can be photographed. This results in a diagram that is particularly sensitive to pressure changes and well adapted to use in the timing of the valves and the spark. It is possible to design the parts just mentioned with such robustness that they will withstand rough treatment and yet be free of inertia and elastic effects that would introduce motions characteristic of the indicator. The motions are, as they should be, characteristic of the pressure-changes taking place in the cylinder.

I have plotted this diagram in terms of the rate of variation of pressure at the bottom of Fig. 7. It is a graph of the tangent to any point on the curve in the upper portion of Fig. 7. Maximum pressure that appears at *t* in the upper portion appears on the lower diagram as the point at which the rate of change of pressure is zero where it crosses the time axis. It is easy to time the arrival of maximum pressure on a (dp/dt) t diagram and it is not so easy to do this on a pressure-time diagram. All of the timing operations become very much more delicate if the indicator is recording the rate of pressure rather than the pressure itself.

Fig. 8 shows a typical curve produced by an indicator that records rates of variation of pressure with time. Compression begins at A_1; the point at which the compression has risen to nearly a maximum value and where the spark occurs is at B_1. The rate of rise of pressure becomes enormous at C_1 and the maximum pressure comes

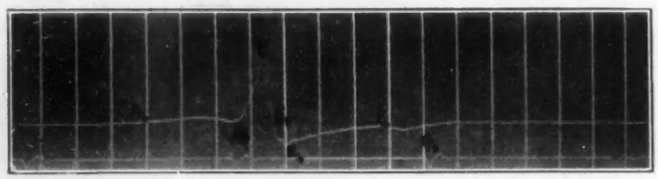


FIG. 8—A TYPICAL INDICATOR-CARD PRODUCED BY AN INSTRUMENT THAT RECORDS THE RATE OF VARIATION OF THE PRESSURE WITH THE TIME

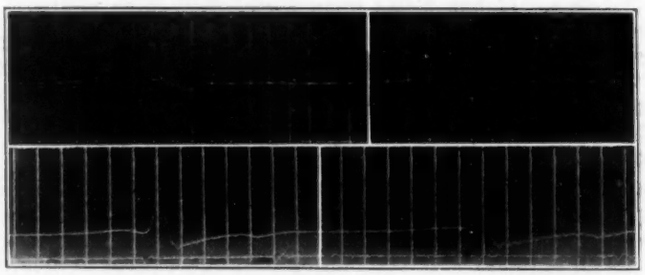


FIG. 9—DIAGRAMS OF A SERIES OF OBSERVATIONS IN WHICH THE SPARK-TIMING WAS VARIED

In the Upper Left Diagram the Spark Was Greatly Retarded and Was Gradually Advanced in the Diagrams in the Upper Right and Lower Left Corners until the Maximum Advance, Which Was Much Too Far, Was Attained in the Diagram in the Lower Right Corner

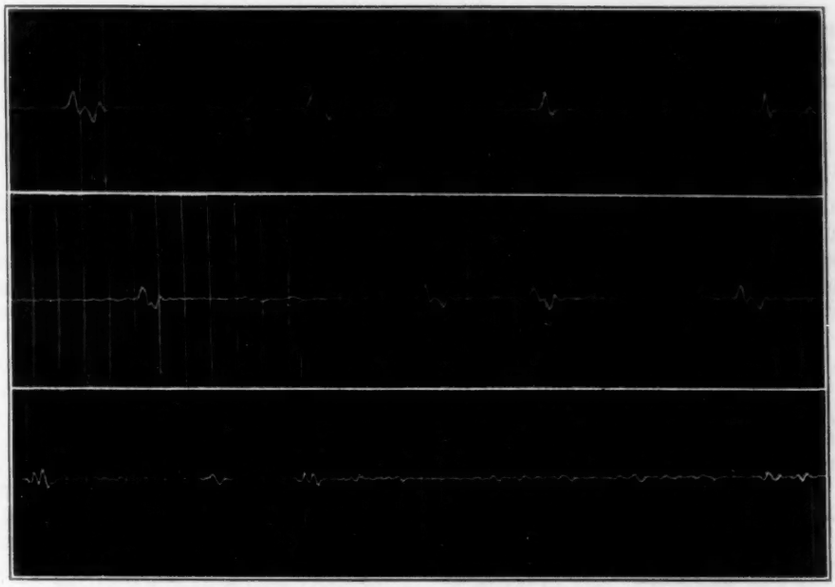


FIG. 11—AN INDICATOR RECORD OF THE ACTION OF THE FRONT END OF AN ENGINE CRANKSHAFT AT SPEEDS (FROM TOP TO BOTTOM) OF 600, 425 AND 700 R.P.M.

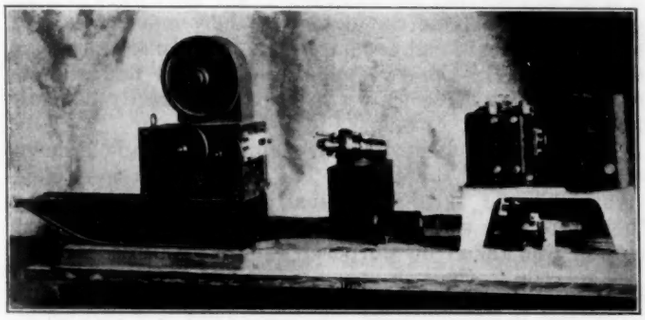


FIG. 10—THE PHOTOGRAPHIC RECORDING APPARATUS AS SET UP TO INVESTIGATE THE SOURCES OF NOISE IN ENGINES THAT ARE RUNNING

at D_1 where the trace crosses the time axis. The maximum rate of fall of pressure comes at E_1 and the opening of the exhaust-valve occurs at F_1. This causes a sudden decrease of pressure that is shown between F_1 and G_1. The out-center position is shown at G_1. The lower portion of Fig 8 shows the positions of in-center and out-center of the piston.

The diagrams in Fig. 9 represent a series of observations beginning at the upper left-hand illustration, where the spark is very much retarded, and ending at the lower right-hand diagram with the spark much too far advanced. The record at the lower edges of all of these diagrams gives the time of in-center. Thus, in the first diagram, the spark occurred considerably after in-center and the pressure rise due to combustion was hardly more than the compression pressure. The fall in pressure when the exhaust-valve opens is very rapid, because burning was still going on due to the lateness of the spark.

The diagram in the upper right-hand corner shows the effect of advancing the spark. The pressure is rising closer to in-center and the amount of "dump-out" of pressure at the time the exhaust-valve opens is growing less. The spark is advanced still farther in the lower left-hand diagram and the rate of pressure-rise becomes greater. The dump-out of pressure at the opening of the valve is no more than that shown in the upper right-hand diagram, although the pressure has been higher. The beginning of vibrations in the gas can now be noticed. The rising portion of the curve after in-center was very smooth in the upper right-hand diagram, but in the lower left-hand diagram it is beginning to be broken into vibrations of about 500 to 1000 per sec. There is the beginning of a knock coming into the explosion. The spark in the lower right-hand diagram is set very much advanced, purposely, and the effect of the knock, which is enhanced, can be seen.

These diagrams are presented to give an idea of the

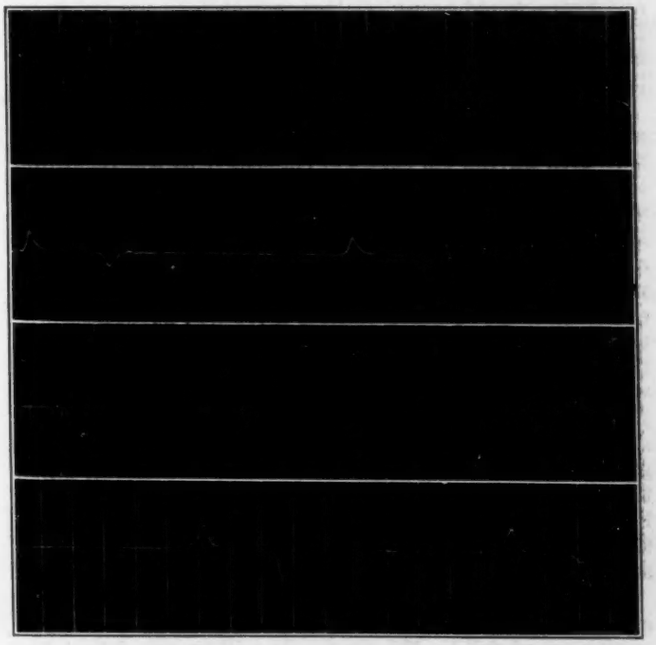


FIG. 12—A SIMILAR RECORD OF VALVE ACTION

In the Top Diagram the Rocker-Arms Were Held on the Tappets: in the Next Everything Was Adjusted Tightly; the Next Record Was Obtained with a Loose Adjustment between the Rocker-Arms and the Tappets and the Bottom Diagram Shows the Effect of Holding the Rocker-Arms Tightly on the Valve-Stems

kind of record that one can obtain. I have taken them at engine speeds as high as 2500 r.p.m. The scale is ample to afford a clear study of the effects of changes of mixture, spark, load, engine speed and other features of interest.

Anti-Knock Research

Thomas Midgley, Jr., has been using this particular indicator recently and Figs. 10 to 12 illustrate his use of it in studying the sources of noise in engines that are running. He has recorded two sets of data in this manner. In obtaining the data shown in one set, he was interested in studying the action of the valve and that around the camshaft. In the other set, the behavior of the crankshaft in regard to chasing and wobbling in its bearings was studied, with engines that he reports as having been thought to be in the pink of condition.

Fig. 10 shows his set-up of the entire apparatus. The recorder is shown on the table at the center of the illustration. One advantage of a self-contained recorder of this sort is that the apparatus can be shaken without shaking the parts relatively to each other; whereas, if a beam of light is thrown out from one support upon a screen that has a separate support, all of the records exhibit wiggles in the lines. In such a case one cannot be certain whether these wiggles are due to legitimate reasons or to the general vibration of the apparatus.

Fig. 11 is intended to show the effect of chasing motions of the crankshaft itself. The apparatus was arranged so that a wheel was carried on the end of the crankshaft. Fastened by springs to that wheel, exterior to it but concentric with it, was a heavy piece having a large amount of inertia. This was provided with brushes to carry off electrical currents and these were conveyed to the recorder.

The idea is that, since the crankshaft is turning around, if it were turning at a uniform speed it would carry this large inertia-piece with it at a uniform speed, and there would be no relative motion between the floating, heavy, inert piece and the crankshaft itself; but if the crankshaft is accelerating and retarding, there would be relative motion between it and the inertia-piece.

It was arranged so that, as the two moved with reference to each other, a small magnet was plunged into a tiny coil, having its contacts connected to the recording instrument; hence, any relative motion that occurred would be recorded as wiggles in the lines of the record. If one were dealing with a steam turbine, one would have a perfectly straight line down through the middle of the record, but if the power comes on in spurts as it does even in a multi-cylinder engine, there will be an acceleration of the crankshaft, it will again chase ahead of the floating piece, the floating piece will tend to catch up with it and there will be indications as shown in Fig. 11. For example, at the top of Fig. 11 is evident a very good sharp explosion from the engine followed by another good explosion, but there was evidently something extra that came in there, some extra wiggle that was not caused by the explosion, and neither one of the two explosions on the left is particularly sharp. They show extra vibration and an extra source of noise for some reason.

The top curve in Fig. 11 shows the engine when idling at 600 r.p.m. It was then put under full load and the engine speed fell to 425 r.p.m. It will be noted that the engine was missing fire, as shown in the middle curve but, in addition, under a heavy load, such sharp breaks do not appear as when under no load at all. The engine was then run idle at a higher speed, approximately 700 r.p.m. Once in a while there are sharp breaks and from time to time there are extra breaks. Evidently, there is a source of some vibration other than that due to the mere blow of the explosion each time. The engine is over-running. The amount of over-run and chatter can be measured with fair accuracy from the shape of the curve. It shows up best in the first wiggle to the left in the bottom curve.

The other test that Mr. Midgley made was on valves and rocker-arms. A small piece of soft iron, made adjustable, was carried on the rocker-arm that came into the field of a permanent horseshoe magnet. A few turns of wire around the magnet were connected with the recorder. Every time the rocker-arm moved in the field of the permanent magnet, a current was induced in the coil, this, in turn, setting the tungsten wire in the field of the galvanometer to moving and producing a wiggle on the record.

A number of tests are shown in Fig. 12. The rocker-arms were held tightly on the tappets. The top view represents the rise and fall, showing a very regular motion free from chatter. In the next view, with everything adjusted as tightly as possible, Mr. Midgley got a particularly small and sharp uniform motion of the parts; in the third view he had the adjustments rather loose, purposely. Comparing this last with that at the top, it will be noticed that the lost motion has begun to show up quantitatively. One can determine how much it is and how long it lasts and compare good adjustments such as those shown in the two upper views with bad adjustments such as those shown in the view directly beneath them.

The view at the bottom shows an attempt to hold down one end and not the other. The rocker-arms were held tightly on the valve-stems just as in the second view they were held tightly on the tappets. Again, when lost motion was taken up purposely, there was less chatter. The records in the two upper views and that at the bottom show a very good performance compared with that in next to the bottom view, but the record in the view directly above it is the best.

The records I have presented here do not include those from bad-acting valves or engines generally and so they do not show the worst kinds of trouble that one can get. They give an indication of what one can work toward.

THE DISCUSSION

J. H. Hunt:—What is the natural period of that vibrating element? What is its sensitivity in regard to current? I can use it in an entirely different way from the one Professor Trowbridge has outlined.

Prof. A. Trowbridge:—One can have any free period desired; it depends upon the tension. I have been using it so that it had no period at all. The damping is really of more importance than the question that Mr. Hunt raises. I had considerable trouble with that. The instrument that I designed for use in the Army was not properly damped; when I returned home I redesigned it so that I have a practical instrument. It is heavily damped.

In regard to its current sensitivity, I usually work with a deflection of about 10^{-4} amp. per mm. (0.03937 in.). The resistance of the tungsten wire itself is in the neighborhood of 15 or 20 ohms. The wire should be made as short as possible. Most people are wrong in that. The string should be made as short as possible because the damping depends upon its length. Damping is much

(*Concluded on page* 368)

Molecular Movements During Combustion in Closed Systems

By THOMAS MIDGLEY, JR.[1]

PENNSYLVANIA SECTION PAPER *Illustrated with Photograph and Charts*

THE paper is an exposition of the theoretical analysis made by the author of the experimental work of Woodbury, Canby and Lewis, on the Nature of Flame Movement in a Closed Cylinder, the results of which were published in THE JOURNAL in March, 1921. No experimental evidence is presented by the author that has not been derived previously by other investigators.

The relation of pressure to flame travel is derived first, the relation of mass burned is considered and a displacement diagram constructed, described and analyzed. The break of the flame-front curve, called the "point of arrest," enters prominently into the discussion and computations; the pressure in the flame-front is studied; the reaction-velocities are calculated; and general comments are made.

The mathematical analysis is supplementary to the paper itself and includes the calculations for the relation of pressure to flame travel, pressure drop in the flame-front, and the actual and the theoretical reaction velocities.

THIS paper presents a method of analyzing the behavior of all portions of a gas as it undergoes combustion in a closed chamber, a photograph of the flame-travel and a pressure-time card being used as a basis for the analysis. It is shown how the movements of these gases are affected by the progress of the flame-front. The physical forces set up by combustion are disclosed and the actual reaction velocity of combustion is derived. The paper is purely analytical. No experimental evidence is presented that has not been derived previously by other investigators. The particular piece of experimental work that is used for the basis of this analysis is that carried out by Woodbury, Lewis and Canby, at the Eastern laboratory of E. I. du Pont de Nemours & Co., and presented in their paper on The Nature of Flame Movement in a Closed Cylinder,[2] at the 1921 Annual Meeting of the Society.

An outline of the procedure followed by Woodbury, Lewis and Canby in their work is that a bomb 1 ft. long and 4 in. in diameter was arranged so that an explosive mixture of gases could be fired inside it. A narrow window of the same length as the bomb was provided in the wall of the chamber for emitting the light from the reaction, and a system of lenses was used for focusing an image of the window upon a highly sensitive film mounted upon a revolving drum. A pressure element was inserted at the center of the top of the bomb for measuring the rise of pressure. The pressure changes were recorded on a film that was held on a drum mounted upon the same shaft as that of the drum for recording the flame travel as is shown in Fig. 1. By developing the two films, a record was obtained which gave a distance-time curve of the flame travel and the pressure-time curve corresponding thereto. Two typical curves of this sort are shown in Fig. 2. Ignition was obtained from a spark-plug located in the center of the bottom of the bomb. For purposes of analysis in this work, the bomb will be considered as being of unit cross-sectional area, instead of being 4 in. in diameter; this simplifies the calculation and introduces no errors. The actual pressure curves obtained by Woodbury, Lewis and Canby will not bear the scrutiny of this analysis, due most likely to improper calibration. Mr. Woodbury concurs in the belief that improper calibration of the pressure element may easily have been possible, as accurate pressure measurements were considered secondary to the photograph-

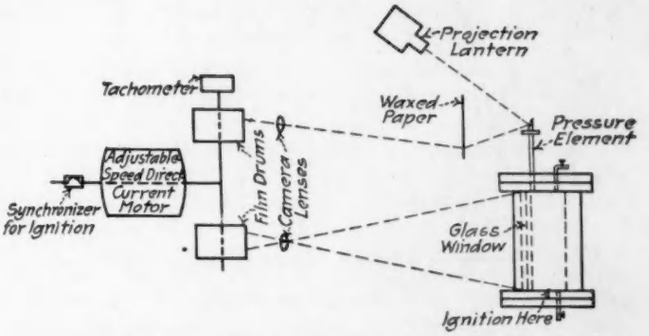


FIG. 1—ARRANGEMENT OF APPARATUS TO GIVE A DISTANCE-TIME CURVE OF FLAME TRAVEL AND THE CORRESPONDING PRESSURE-TIME CURVE

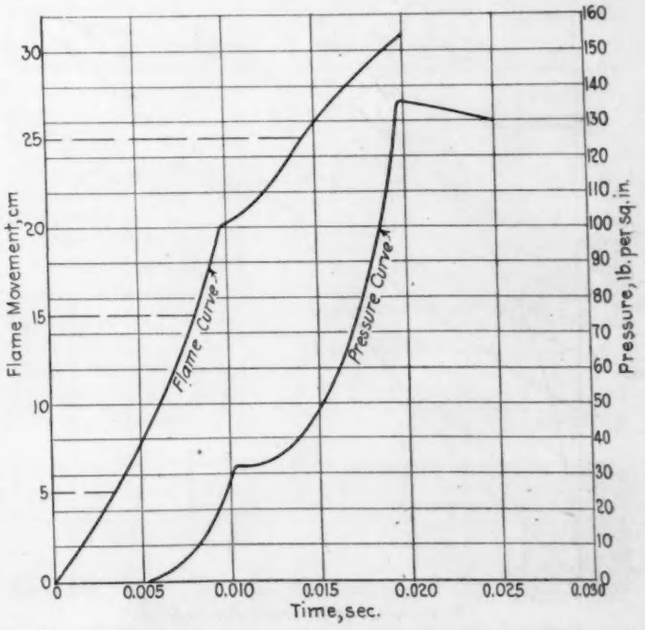


FIG. 2—TYPICAL DISTANCE-TIME CURVE OF FLAME TRAVEL AND THE PRESSURE-TIME CURVE THAT CORRESPONDS TO IT

[1] M. S. A. E.—Chief Engineer, fuel section, General Motors Research Corporation, Dayton, Ohio.

[2] See THE JOURNAL, March, 1921, p. 209.

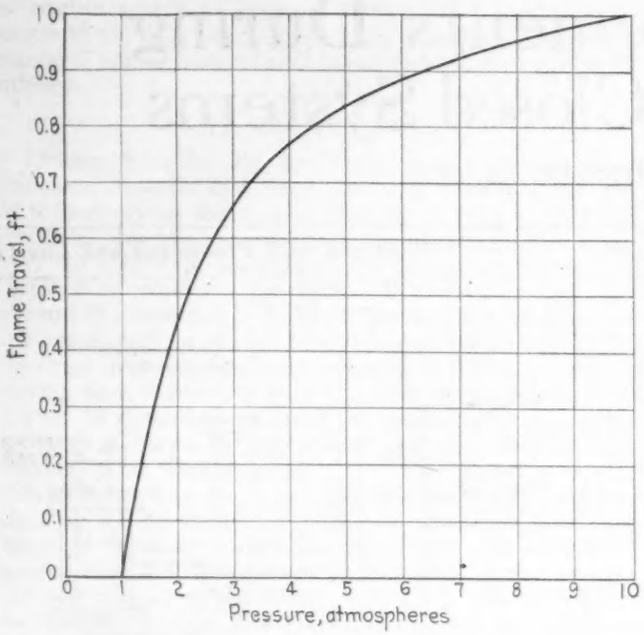


FIG. 3—CURVE SHOWING THE RELATION BETWEEN FLAME TRAVEL AND PRESSURE

ing of the flame-front. I have therefore taken the liberty of substituting a "typical" curve for an "actual" curve in Fig. 2, to avoid incongruous results.

RELATION OF PRESSURE AND MASS BURNED TO FLAME TRAVEL

The first essential in this analysis is to derive the relation of the pressure to the flame travel. This derivation is very simple; it is simply necessary to refer the two curves of Fig. 2 to each other, making allowance for the transmission of pressure from the flame-front to the pressure element at the velocity of sound. This result is plotted in Fig. 3.

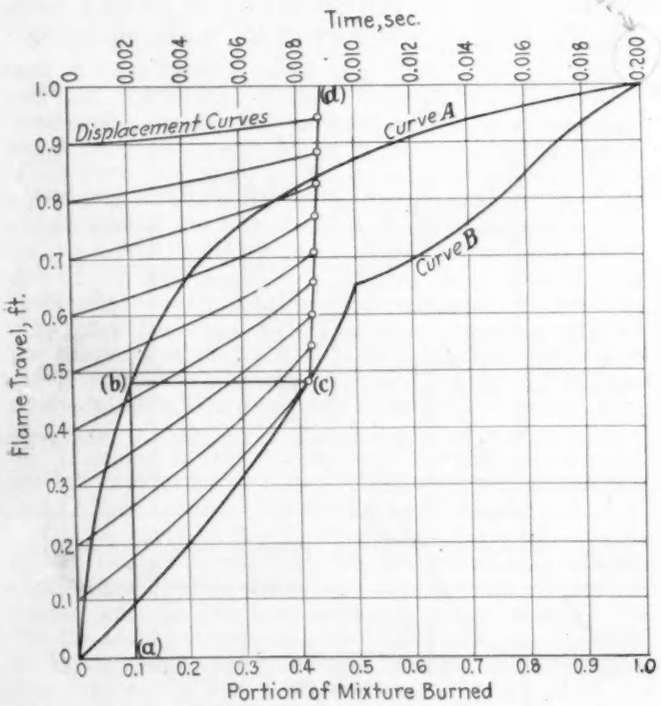


FIG. 4—DISPLACEMENT CURVES SHOWING THE RELATION OF THE MASS BURNED TO THE FLAME-FRONT TRAVEL

The next step in the analysis is to obtain the relation between flame travel and the percentage of gas burned. This is obtained by considering the compression of the unburned portion of gas in advance of the flame-front. This gas either compresses adiabatically or receives heat from some source. The time is entirely too short for any appreciable interchange of heat with the walls of the container, the only possible change of total heat being therefore by radiation from the luminous portion of the charge. Infra-red spectroscopic investigations do not show any great absorptive power of the unburned portion. There is, of course, some absorption, but it is altogether negligible for present purposes. We are therefore fully as well justified in assuming that this gas compresses adiabatically as we are in assuming the practical existence of adiabatic compression itself.

We can now proceed. The volume of the unburned portion at any instant is established by the position of the flame-front; the pressure corresponding thereto is obtained from Fig. 3. The original pressure and specific volume of the charge are known; so, by using the equations of adiabatic compression, we can calculate the quantity of unburned charge, which, by difference, gives us the total quantity of charge burned. This is graphically represented as Curve B, in Fig. 4.

Before proceeding with the main discussion, it is well to point out that this method of analysis makes it possible to determine the thermal behavior of the hot gas at the rear of the flame-front, which is also being compressed, and to determine the nature of this compression. We are justified in making no assumptions regarding this gas except that the general equation for its compression should be of the type $PV^n = C$, where n is always less than the ratio of the specific heats. To investigate this equation properly, it will be necessary to have much more accurate data than are now available. But we are able from the data at hand to predict that n is variable in the equation $PV^n = C$, and a function of flame travel, x. A critical study of $n = f(x)$ undoubtedly will throw much light on the questions of after-burning, dissociation and heat rejection to cooling systems. These subjects form no part of the present paper, which is concerned only with the physical behavior of the gases, and further discussion of them must be left to a later date.

DERIVATION OF DISPLACEMENT DIAGRAM

A very interesting diagram can now be derived. If a flame-front photograph be plotted on the same coordinate sheet, Curve B in Fig. 4, and the corresponding relation of mass burned to flame-front travel, given by Curve A in Fig. 4, it is possible to determine the time and place that a certain mass-particle burns. For example, to determine at what point on the flame-front curve a particle that was 0.1 of the way up the bomb before ignition undergoes combustion, we start at a, which represents 0.1 of the total mixture by weight, and draw a vertical line until it intersects Curve A at b. From this point we draw a horizontal line until it intersects Curve B at c. Thus, c is then the point at which a particle with an original position of 0.1 is undergoing combustion; and, if the distance from c to d be divided into nine equal parts, the angle of this line being laid off with relation to the velocity of sound, it will give the corresponding positions of particles at 0.2, 0.3 and so on. Exactly the same procedure can be employed for determining the positions of particles at the rear of the flame front, and

the result is the displacement diagram of the particles during total combustion that is shown in Fig. 5.

Study of Displacement Diagram

The displacement diagram thus developed is plotted between the coordinates of distance of flame travel and of time. The velocity of a particle at a given point can be determined from such a diagram by measuring the tangent of the displacement curve. It is this fact that makes it possible to determine the causes of many of the phenomena that were first brought to light by the work of Woodbury, Lewis and Canby. For example, there is a vibratory condition in the gases following complete combustion, which is shown in Fig. 6 and exhibits itself as successive light and dark bands. It was demonstrated by Woodbury, Lewis and Canby that these light and dark

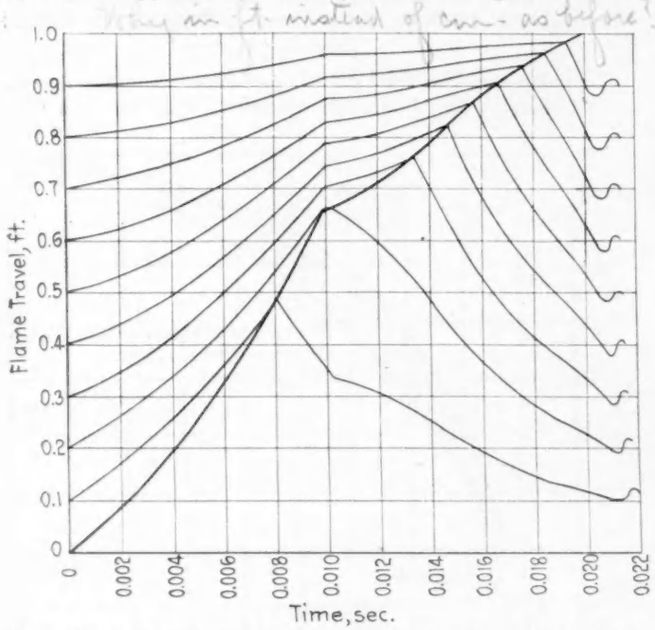


Fig. 5—Displacement Diagram of the Particles during Total Combustion

bands corresponded to a vibratory movement on the pressure record. The expansion line of an indicator-card from an engine shows precisely the same thing, as illustrated in Fig. 7, and is thus proved to be due to a surging of the gases. The displacement diagram gives a complete explanation of the mechanism by which these "surges" are set up. It will be observed from the diagram that, after the flame-front has reached the end of the bomb, the mass-particles have a rearward velocity. Since these particles have inertia, surging consequently results as is indicated in Figs. 5, 6 and 7. The pressure of the surges can be calculated.

Another very interesting disclosure is the condition resulting from the break of the flame-front curve, called the "point of arrest." Sudden changes occur in the velocities of the mass-particles at the point of arrest, this being evidenced by the sharp breaks in the displacement curves. Because of these changes in velocity and the fact that the particles have inertia, wave motion should result. The reproduced photograph in Fig. 6 shows that this is the case.

A further point of interest is the bright flash on the photograph extending from a point at approximately the position of the flame arrest toward the bottom of the bomb. This has heretofore been interpreted as evidence of a recombustion running *rearwardly* through a portion

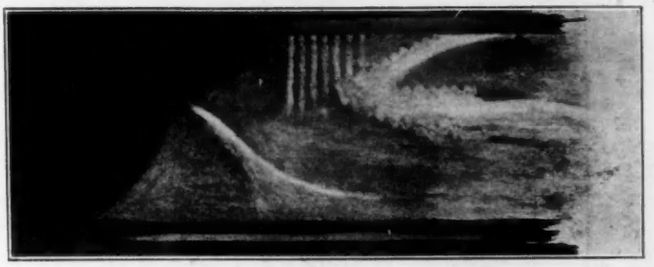


Fig. 6—Proof of the Existence of Vibratory Conditions in the Gases Following Complete Combustion Is Afforded by the Presence of the Successive Light and Dark Bands in a Photograph

of the mixture that remained unburned during the passage of the flame-front. But the displacement diagram in Fig. 5 shows conclusively that this is not the case; because, if it were due to a chemical reaction, it could not remain stationary with respect to the mass-particles, but would be forced to travel through them. On the other hand, if it were a point of extreme luminosity, it *would* remain stationary with respect to the mass-particles. Since this last is precisely what does happen, it proves that the bright flash is simply a portion of the burned mixture that is more luminous than the adjacent material.

Pressure in the Flame-Front

Attention is called to a very interesting fact that the displacement diagram in Fig. 5 gives us, which deserves serious consideration because it is a new thought about flame propagation. If the path of any mass-particle be followed on the diagram, it will be observed that the particle has an upward velocity just before it is burned; but, immediately after its combustion, the same particle has a downward velocity. In view of the facts that the particle has mass and that its velocity has been changed, a force must have been operating in the direction of the change of velocity. In a space filled with gas alone, the only forces that can be operating are such gas pressures as may be due to unequal distribution of pressure within the container. It is possible to determine just what the pressures in the flame-front are by using some equations of flow; this is explained in Appendix 2.

Fig. 8 shows a curve of these calculated pressures in the flame-front during the flame travel. The pressure shown at any point is the difference between the pressures of the unburned and the burned portions of the charge, the unburned portion being at the higher pressure. It is interesting to note that this pressure is merely nominal through the early stages of the reaction; but, just before the point of flame arrest, it has become an appreciable quantity. I wish to point out that the pressure differential would have become enormous if the flame-front had not been "arrested." This consideration will be referred to later in this discussion.

The next item of interest that can be derived from a displacement diagram is the reaction velocity of the combustion. The reaction velocity is a value representing

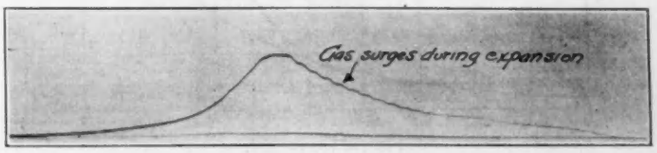


Fig. 7—Photograph of an Indicator-Card Obtained from an Internal-Combustion Engine Which Shows Evidence of Gas Surges Similar to Those Illustrated in Fig. 6

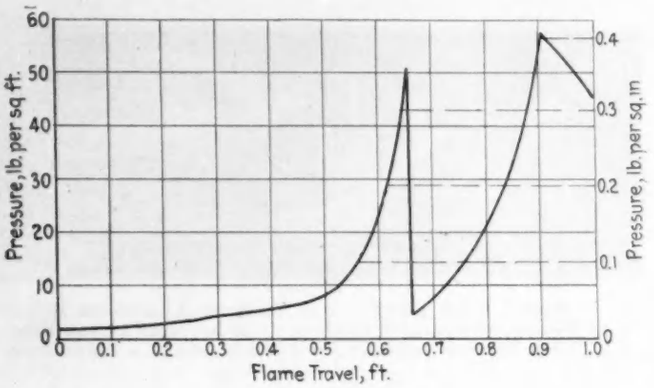


FIG. 8—CURVE OF CALCULATED PRESSURES IN THE FLAME-FRONT DURING FLAME TRAVEL

the number of molecules entering into combustion in a unit length of time. It can be obtained by scaling the upward velocity of a mass particle just before combustion, subtracting the value so obtained from the velocity of the flame-front, and multiplying the remainder by the density of the unburned gas. This is discussed in Appendix 3. The actual reaction velocity is shown in Fig. 9.

It is unfortunate that the du Pont apparatus gave the "flame arrest" phenomena, which we feel must be attributed solely to a harmonic condition of the bomb, as otherwise it would have been possible to continue the reaction-velocity curve to the end of combustion, then to have plotted this as a function of the pressure and to have derived the exponential equation of their relationship. With this information it would be possible to tell exactly just how the various molecules and atoms concerned were behaving during combustion. Unfortunately, the flame

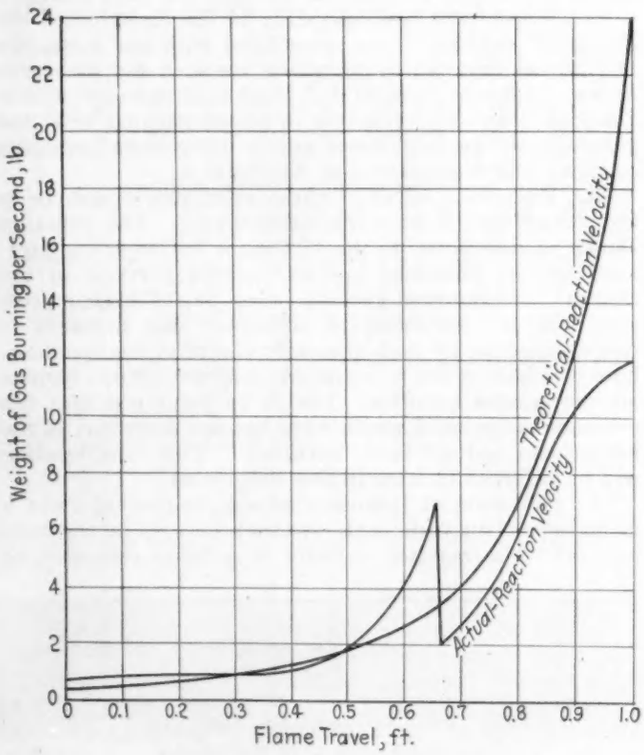


FIG. 9—CURVES SHOWING THE ACTUAL AND THEORETICAL REACTION VELOCITIES OF COMBUSTION

arrest prevents this most interesting analysis and we must wait until this harmonic condition is removed.

GENERAL COMMENTS

I wish to emphasize that the pressure differential at the flame-front suggests itself very strongly as the physical cause of the effect that we know in practice as the fuel knock or, more properly, as detonation. If this point is accepted, calculations can be carried further to illustrate practically all of the phenomena which accompany detonation.

We hope to continue these investigations to further conclusions and to present the results obtained from time to time as the various stages are completed.

APPENDIX 1

CALCULATION OF THE RELATION OF PRESSURE TO FLAME TRAVEL

To calculate the relation of pressure to flame travel, we proceed in the following manner. Fig. 10 is a dia-

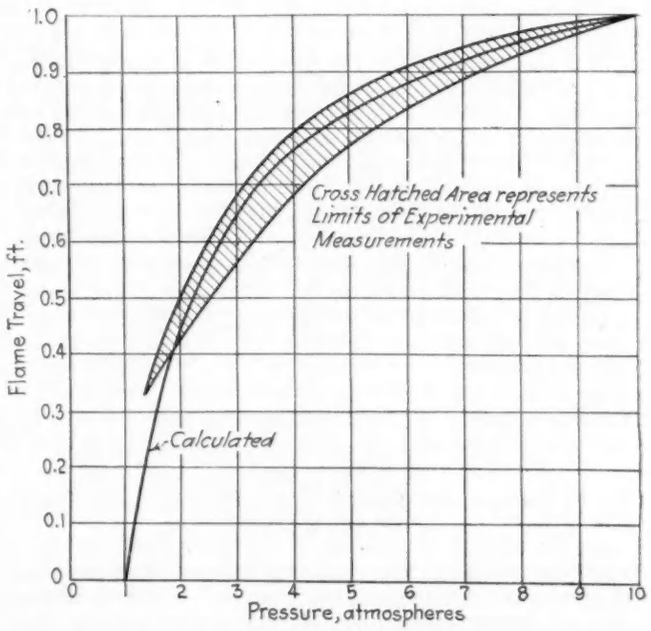


FIG. 10—CURVE OF THE RELATION BETWEEN FLAME TRAVEL AND PRESSURE IN A CLOSED BOMB

gram of a closed bomb with a flame-front at a distance x from its origin.

Let

x = the distance the flame has traveled
B = the original volume of gas that has expanded to x
p = the instantaneous pressure
P_1 = the original pressure = 1 atmosphere

By simple adiabatic relationship,

$$\frac{p}{P_1} = \left(\frac{1-B}{1-x}\right)^{\gamma}$$

and

$$P_1 = 1$$

Then,

$$p = \left(\frac{1-B}{1-x}\right)^{\gamma}$$

This can be transposed to

$$B = 1 - (1-x)\,p^{1/\gamma}$$

By using values from Curve A in Fig. 4, we can derive values for B and plot Curve B of Fig. 4.

APPENDIX 2

Calculation of Pressure Drop in the Flame-Front

To calculate the pressure drop in the flame-front, let us consider the flame-front as being stationary and that the gases pass through it. This is perfectly allowable for instantaneous considerations such as the present. We then have the condition shown at the left of Fig. 11, where the velocity, v, is the velocity of the particles entering combustion with respect to the flame-front. Let dm be a small mass of gas in the flame-front, of a thickness ds and an area equal to unity. Then the pressure change is equal to $-(P_1 - P_2)$, which is equal to $-dp$. It will be observed that, as dm passes through the flame-front, it increases its temperature and consequently its volume. An increase of volume requires an increase of velocity, and an increase of velocity means a pressure drop to secure the necessary acceleration. Since force equals mass times acceleration, or $F = MA$, we have, for unit area, $-dp = dmA$. But

$$A = \frac{dv}{dt}$$

where

v = the velocity

Then,

$$-dp = dm\frac{dv}{dt}$$

It will be observed that

$$dm = \frac{ds}{gV}$$

where

V = the specific volume

$g = 32.2$

Substituting for dm we get

$$-dp = \frac{ds}{gV} \times \frac{dv}{dt}$$

But

$$\frac{ds}{dt} = v,$$

Then,

$$-dp = \frac{vdv}{gV} \qquad (1)$$

Let W be the weight of gas entering the flame-front, expressed in pounds per second. Here W is treated as a constant on the assumption that during the passage of the element dm through the flame-front no change in W occurs. Of course, this is not strictly true. The right to assume constancy depends largely upon the magnitude of the actual change of W during this time, which will depend upon the length of time the element dm is in the flame-front and this, in turn, depends upon the thickness of the flame-front. Woodbury, Canby and Lewis have established that the maximum pressure-rise occurs as nearly coincidently with the end of the flame travel as it is possible to measure. If the flame-front were of any appreciable thickness, this condition could not exist. I have some inconclusive evidence of another sort, the details of which are unnecessary here, that indicates a thickness of flame-front during detonation of less than 1/32 in. I believe this to be a safe value to take for the flame-front thickness. It introduces, at the point of arrest, a deviation from constancy of only 1 per cent for W. Under these conditions the assumption is justifiable. Since the bomb is of unit cross-section, $v = WV$; then $dv = WdV$. Substituting in equation (1) for v and dv, we obtain

$$-dp = \frac{W}{g} \times WdV = \frac{W^2}{g}dV$$

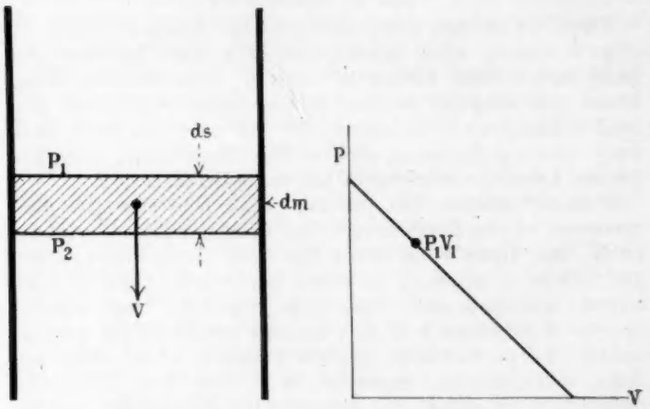


Fig. 11—Diagram (at the Left) of the Relations Existing in the Flame-Front Where the Gases Are Considered as Passing through a Stationary Flame-Front and (at the Right) Showing How the Equation for Pressure Drop Becomes a Straight Line under Certain Conditions

By integration between the limits P to P_1 and V to V_1 we obtain,

$$P_1 - P = \frac{W^2}{g}(V - V_1) \qquad (2)$$

In equation (2), P_1 and V_1 are constants; P and V represent the pressure and the specific volume. This equation becomes a straight line on the pressure-volume diagram, as shown at the right of Fig. 11.

We can now proceed to apply this formula to calculate the pressure in the flame-front. For example, considering a particle having an original position of 0.200 that burns at position 0.663, at that time it has a velocity with respect to the flame-front of 11 ft. per sec., which we obtain by scaling the displacement diagram. The original specific volume of the gas is 12 cu. ft. per lb. The specific volume of particle 0.200 at position 0.663 *before* combustion is $12[(1 - 0.663) \div (1 - 0.20)] = 5.06$ cu. ft. per lb. The specific volume of particle 0.200 at position 0.663 *after* combustion is $12(0.663 \div 0.200) = 39.9$ cu. ft. per lb. Hence, the weight-rate of gas flowing is $11 \div 5.06 = 2.17$ lb. per sec. Substituting these values in equation (2), we have $P_1 - P = [(2.17)^2 \div 32.2](39.9 - 5.06) = 5.09$ lb. per sq. ft.

APPENDIX 3

Derivation of the Actual-Reaction Velocity

To derive the actual-reaction velocity, take the velocity of the particles with respect to the flame-front, as obtained from the displacement diagram (Fig. 5) and divide this by the specific volume. We obtain thereby a figure representing the number of molecules passing into the combustion per unit of time. This is simply W, the weight of gas passing into the flame-front per second, which was derived in Appendix 2. There values are plotted against flame travel in the curve of actual reactional velocity shown in Fig. 9.

As an extrapolation, for the reaction velocity of particle 0.0, we have burned 0.195 cu. ft. in 0.01 sec. at the point of flame arrest, and 0.195 cu. ft. $\div$ 12 = 0.01625 lb. Then the average reaction-velocity has been $0.01625 \div 0.01 = 1.625$ lb. per sec. Therefore, the initial-reaction velocity should be in the neighborhood of 0.8. We can assume this for purposes of plotting and then check by the integration of the curve. This value checks well enough for all practical purposes.

THE DISCUSSION

PROF. AUGUSTUS TROWBRIDGE:—Mr. Midgley shows in Fig. 5 exactly what takes place in a way that has perhaps never been presented before. The obvious thing about this diagram is that at the flame-front there is a real reflection, so to speak, of the particles that have been moving forward, and at the flame-front they are turned around and move in the opposite direction.

I do not doubt that the first man who saw the phenomenon of the flame arrest was greatly puzzled. Apparently, the flame runs down the tube toward the closed end; then it appears to come backward; then it goes ahead, wavers again, then goes forward; then wavers two or three times and may become extinguished at that stage. In particularly narrow tubes it often does become extinguished; however, if it does not, it always proceeds with something approaching detonation speeds, speeds inordinately greater than those shown in Fig. 5. One thing that occurs to me is that in the conditions shown in Fig. 5 we are probably far from those of the detonation stage. If the experiment had been carried out in a tube of narrower bore and greater length, I should judge that there might have been more than one of these stages of flame arrest, followed by a curve which would be almost a vertical line.

It is highly desirable that accurate data be collected so as to permit a rigid test of the validity of the relationship $pv^n = C$ to the rear of the flame-front. Mr. Midgley states that the interpretation of the data already at hand suggests that n is not a constant. If this be so, the process can hardly be described as adiabatic. If n is variable, the process is one in which heat is being lost by the system and so a knowledge of how n varies with the time or with the position of the flame travel may throw light on the manner of the heat loss.

I recorded some results of experiments about a year ago, at a meeting of the National Academy of Sciences, regarding changes of mixture, content and pressure. I am reasonably certain that there is absolutely no change possible in flame speed brought about by change of external pressure. If conditions are started at a pressure of 1 atmosphere, we have a certain speed and we get practically the same speed at 10 atmospheres. Neither do changes in gross temperature outside the cylinder do very much. The speed is very nearly the same, whether it starts hot or cold. The conclusion is that we cannot get close enough to the real thing. With this wave-front with the change in pressure, the temperature at that point is highly effective in changing the flame; but we must do it chemically, physically, through reagents that get in close, such as sound-waves and things of that sort, and not from the outside.

I applied the experiments to very long tubes compared to those used in Mr. Woodbury's work and observed the very curious phenomenon that in practice, in closed-bomb experiments, this flame wave is very much longer than we imagine. I have some fairly conclusive data that we can actually fire a mass of coal gas and air in the ratio of 14 to 1, and can fire that mass much more rapidly by starting it off 22 in. away from the bomb than by firing it in the bomb itself. We get complete combustion more quickly by getting past this stage of wavering, and shooting the charge in then. One will find that the pressure-rises actually come more rapidly by getting off farther, so that this stage of wavering is passed, and then coming in at almost detonation speed; that is, one gets higher gas-flame speeds from detonation from a greater distance.

C. A. WOODBURY:—Those who have spent much time in theorizing on flame propagation realize the necessity for a simplification of the conception of what takes place when flame travels across a cylinder-head. The displacement diagram that Mr. Midgley shows in Fig. 5 is a contribution along that line. It will simplify our ideas of just what happens when we ignite an explosive charge of gas and air and the flame travels across the cylinder. But there is one point which is not clear to me in regard to his conception of this flame movement. He speaks of the difference in pressure between the area ahead of the flame and that back of it, and that it is this difference in pressure that is responsible for the rapid movement of the particle from the flame-front when once released. This particle is ahead of the flame front before the flame hits it and ignites it, and of course it must be behind it after it is burned. When this flame moves forward and attacks a particle of gas and burns it, that particle of gas must expand, and something must happen to it. The heat units liberated require that this gas particle expand, but that expansion will develop pressure both ahead and back of the flame-front. The particles ahead will be pushed forward still farther and the particles behind will be pushed back still farther, the extent of the movement depending in both cases upon the relative compressibility of the two sections of gas. I cannot see that this needs the assumption that we must have a pressure differential between the areas ahead of and behind the flame-front.

THOMAS MIDGLEY, JR.:—It is because of the law of nature that force equals mass times acceleration. The particle has mass, and it is accelerated toward the rear of the bomb. Hence, there must be a force acting. The only force that can be acting in the space that is filled with nothing but gas is a pressure differential in the gas. The region within which this pressure differential exists is actually in the flame-front. The gas on the forward side of the flame-front in Fig. 5, the point of flame arrest, has about 0.3 lb. per sq. in. higher pressure than the gas back of the flame-front, in order that the flame may be enabled to expel the products of combustion.

The total pressure developed is a large factor in comparison with the pressure differential. The pressure in the gas does not all become pressure in the flame-front by any means. The pressure differential in the flame-front is only the small part that is necessary for the flame to get rid of its ashes. It is an automatic ash-remover by which the flame is enabled to throw the reacted mass-particles to its rear.

MR. WOODBURY:—I conceive that at the point *a* in Fig. 5 the downward velocity of that particle away from the flame-front is greater than the upward velocity of particle *b* in the direction *c d*, but what is the condition down at the bottom of the bomb? Would not the downward velocity of particle *a* be less than the upward velocity of particle *b*, as indicated in Fig. 13?

MR. MIDGLEY:—It makes no difference whether the velocity to the rear is greater than the velocity in front of the flame-front; the fact remains that the direction has been changed. The pressure differential does not apply until the flame-front has had time to get the particles ahead of it started in motion, so that at the bottom, or just at the time of ignition, the pressure differential naturally does not exist. I feel that this disclosure of a pressure differential in the flame-front is the outstanding result of this analysis so far. It is a thing no one has guessed, so it is hard for some of us to conceive.

B. B. BACHMAN: — In considering the Woodbury, Canby and Lewis diagrams of flame movement and the

pressure increment, upon which some of this analysis is based, there is a lag apparent in the time between the initiation of the flame, which starts at zero time, and the initiation of pressure, which is some time later. The pressure curve in Mr. Midgley's analysis apparently allows for some discrepancy that may exist, due to the fact that his theoretical curve of pressure coincides closely with the observed pressures.

In considering the method used in the original investigation, I note that the recording of flame movement is entirely by transmitted light; whereas the rise of pressure, of necessity, must first be transmitted through the contents of the vessel at the velocity of sound before it reaches the pressure element whence the record is transmitted to the film by light rays. This condition raises the question in my mind as to whether the difference in time between the appearance of flame and the initiation of pressure is due to the method of obtaining the record. Have the experimenters done anything to determine what loss in time, if any, exists in transmitting the pressure from the point of inflammation to the instrument?

MR. MIDGLEY:—I think there is no question in the mind of anyone who is willing to think small enough that, with the initiation of the flame-front, there must be a coincident initiation of pressure. But the experimental work does not show it. The theoretical pressure-rise of this flame-front is a curve something like Curve *A* in Fig. 5. It is evident that the flame can travel a considerable distance before there is any appreciable rise in pressure. Although there has been a rise of pressure, it is only a fraction of a pound, due to the fact that only a very small portion of the gas has been burned. One can see that only 0.1 of the gas has been burned when the flame travel is one-half way down the bomb.

MR. WOODBURY:—We did put the pressure element on the base of the bomb. I think it was for the purpose of checking up with the arrest in the flame. It was not simply a surge and the rate of gas burned was exactly the same, even though the flame seemed to stop. We had an idea that the gas was pouring through there. I am not sure whether it checked up with the point that the first indication of the pressure registered.

A. C. BIGELOW:—Was the ignition directly in the bottom of the cylinder?

MR. MIDGLEY:—Yes.

MR. BIGELOW:—Do you consider that there would be very much of a change in the shape of the diagram if that ignition had been on the side?

MR. MIDGLEY:—There might have been; I do not know. This whole pressure differential that is calculated in the Appendices of this paper is on the basis of a bomb of constant cross-section. If we upset the initiation and have a flame-front of a distinct angle, I do not know what the result would be.

G. W. SMITH, JR.:—Have you formed any conclusions as to how the action might be modified for substituting gasoline for acetylene gas?

MR. MIDGLEY:—I have an idea that it would slow down somewhat. Otherwise the diagram would be unchanged.

MR. WOODBURY:—I think that would be the case. This curve of Mr. Midgley's is a typical curve. I think it makes little difference whether that is from and at ordinary temperatures or a permanent gas. That seems to be a typical behavior; only the average rate of the flame would vary.

A MEMBER:—Was anything done in these experiments to use a different moisture-content in the air that was united with these gases for combustion, with the idea in view as to whether, if the moisture-content there contained hydrogen from water, it would have anything to do with the quality of the light bands, if it should reach the firing point of the hydrogen?

MR. MIDGLEY:—I think nothing was done in regard to the moisture-content in the experimental work. There is so much else to be done in that sort of experimentation that this has not been done.

Will Mr. Woodbury subscribe to my belief that we can work more intelligently with this analysis in mind?

MR. WOODBURY:—Without a doubt.

MR. MIDGLEY:—Heretofore, experimentation on flame propagation has consisted more or less of setting up something, watching it burn and then drawing conclusions. There is a little matter of relativity mixed up with it. As we photograph with respect to a fixed bomb, the gaseous molecules in which are moving, the only thing that counts is how fast the molecule is moving with respect to the flame-front. That is, the important thing is a relative velocity of flame-front to molecular movement. Failure to appreciate this point has rendered valueless the large volume of the experimental work of just this sort that has been done.

HARTE COOKE:—In regard to Mr. Midgley's paper, in the old steam-engine days we had a very peculiar pause in the indicator-card steam-line for that type of engine in the low-pressure cylinder. The pressure would come up and the admission would begin; then, strange to say, there would be a pause. We had always thought that this pause was caused by some irregularity in the indicators. We then took cards on the low-pressure cylinder with the high-pressure indicator-motion, bringing the admission in the middle of the card where it could be seen. There was then no question that the pause existed but, at first, so far as we could see, there was no reason it should exist. The valves were opening very rapidly and the piston was standing still. However, as nearly as we could determine, that was at about the place where the temperature of the entering steam became higher than the temperature of the cylinder-walls; it became conscious, so to speak, of the fact that the cold cylinder-wall existed. The cooling effect was so great that the steam was condensed as fast as it came in, momentarily. Then the pressure came up normally. That is interesting, as this pause was very much the same as the pause Mr. Midgley has shown.

THE STUDENT

THE present remarkable position in applied science and in industrial trades of all sorts has been made possible by men who did pioneer work in chemistry, in physics, in biology and in physiology, without a thought in their researches of any practical application. The members of this higher group of productive students are rarely understood by the common spirits, who appreciate as little their unselfish devotion as their unworthy neglect of the practical side of the problems. —William Osler.

INTERNAL-COMBUSTION ENGINE FUELS

THE paper on Internal-Combustion Engine Fuels printed in the March issue of THE JOURNAL on page 187 was read by Professor Norman before the Minneapolis Section somewhat over a year ago. As there was nothing in the paper to indicate this, some readers may have been surprised to find certain discrepancies between the figures used and those given by Dr. J. E. Pogue and others in publications of a later date.

Alcohol for Motor Fuel

NEARLY every general discussion of the problem of motor fuels includes some reference to the use of alcohol for this purpose, either alone or blended with other materials. Engineers have recognized from the beginning the special advantages of alcohol, as attested by the large number of papers and reports on this subject extending from about 1900 to the present time. An exhibition of alcohol engines was held in Paris in 1902. The use of alcohol for power purposes had been developed in Germany under direct Government encouragement to an important industry by 1905. Much of the best and most important experimental work with alcohol has been confined to slow-speed heavy-duty stationary engines, although some attention has been paid to automotive engines.

The special reason for bringing the subject up at this time is not the immediate interest in the use of alcohol as a fuel in this Country in the near future, but rather the trend of development in other countries that may affect very materially the automotive industry here. So long as gasoline is available in the quantities desired at a price that is within reason, the use of alcohol is not likely to become general in engines built specially for gasoline. Moreover, alcohol probably could not be produced in the United States in sufficient quantity to affect the fuel situation here greatly, at a price that would compete with gasoline, unless a very acute shortage of the supply should develop.

There are, on the other hand, localities where quite the opposite condition occurs, the demand for motor fuel being comparatively small, the potential supply of alcohol large and the sources of gasoline distant and controlled by outside interests. This is true to a considerable extent in Cuba, the Philippines, Hawaii and in much of South America, all of which territories are large potential users of the products of the automotive industry. In Cuba at present it is reported that half the motor vehicles are using alcohol. If true, this shows a remarkable demand for a substitute for gasoline as now marketed there. But it may also be a menace to the industry. Existing stock passenger-cars, trucks and tractors are not adapted to the use of alcohol without some important modifications. Its general use under these conditions may lead to difficulties that will tend to discredit the vehicle in which it is used. Our powerplants as they exist today are the result of careful and detailed adaptation to the use of a certain fuel, gasoline. To be equally successful with another fuel of very different character will require another period of adaptation.

In spite of the difficulties and inconvenience that may result from the use of alcohol in present stock engines, both alcohol and its mixtures with gasoline are excellent fuels possessing many important advantages over gasoline. Moreover, the use of them is an economic necessity in some of the countries mentioned above.

Ultimately, the ability of the United States manufacturer to market automotive equipment in the countries must depend upon his producing such equipment adapted to the use of alcohol fuel. The special problems involved in the use of alcohol are many and can only be suggested here, with the hope that some of them at least will be the subject of systematic research and development by some of our laboratories.

The main questions to be considered in the utilization of any new fuel in automotive engines of present types may be stated briefly as follows:

(1) Metering of the charge
(2) Distribution to and in the cylinder
(3) Combustion characteristics within the cylinder
(4) Performance of the engine as regards power, flexibility, and economy
(5) Incidental or secondary effects of the fuel on the engine and accessories

In all these respects alcohol differs so radically from gasoline that a comprehensive research program must include consideration of all of them.

Metering of alcohol, if done in a conventional type of carbureter, will involve differences only in the size of orifice or the metering head necessary for the correct air-fuel ratio. Alcohol in general requires a larger orifice because of both its viscosity and the larger volume used per cubic foot of air.

Distribution of the charge with alcohol as fuel should present no serious difficulties, provided the same degree of evaporation is maintained as with gasoline. Since the boiling-point of alcohol is lower and the heat of vaporization much higher, the amount of heat required in the manifold is a matter requiring experiment. Some recent results show that more heat is required for burning alcohol. The temperature and the amount of heat required to produce a suitable alcohol-air mixture calls for investigation.

The combustion characteristics of alcohol differ radically from those of gasoline. One of the main differences is that starting is more difficult with alcohol than gasoline. Some of the reasons have been pointed out by R. E. Wilson and Daniel P. Barnard.[1] The starting requirements of alcohol must be investigated.

Fuel knock with alcohol will not occur except at very high compression-pressures. This means that the compression-pressure for alcohol can and should be much higher than for gasoline. Pressure all the way from 110 to 180 lb. have been recommended. How much higher this should be for best results is a subject for research and trial.

The range of combustibility is much different from that of gasoline. This is a relation that has a very important bearing on possible fuel-economy. The necessity of using throttle control instead of controlling the fuel supply alone as in the Diesel engine means low efficiency at part throttle. Alcohol is said to ignite at much lower mixture-ratios, so that it may be possible to adopt a method of control by varying the fuel supply alone. This would increase greatly the economy at part load, and might offset the inherent high consumption of alcohol per horsepower. This subject particularly requires laboratory research to determine the limits of combustibility over the full range of pressure and temperature, as well as to show the possibility of controlling power with alcohol by varying the fuel ratio.

The alcohol mixture-ratios that will afford maximum power and economy differ from those for gasoline. Unfortunately we have only very meager data even on the latter. Alcohol mixtures give maximum power when excessively rich; more so than gasoline. The mixtures for maximum economy have

Research Topics and Suggestions

The Research Department plans to present under this heading each month, a topic that is pertinent to the general field of automotive research, and is either of special interest to some group of the Society membership or related to some particularly urgent problem of the industry. Since the object of the department is to act as a clearing-house for research information, we shall be pleased to receive the comments of members regarding the topics so presented, and their suggestions as to what might be of interest in this connection.

[1] See THE JOURNAL, November, 1921, p. 313.

not been well determined; more knowledge here is necessary for the best development of alcohol engines.

Carbonization with alcohol is said to be very small indeed. Is this a fact? The amount of discussion that has been devoted to the subject of carbonization due to lubricating oils would lead one to suppose that the fuel has not much to do with the matter; yet carbonization was not troublesome until motor gasoline acquired an end-point of about 370 deg. fahr. There is room for some real study of the problem of carbonization with alcohol fuel.

Ignition temperatures of alcohol-air mixtures have been studied by Professor Dixon, of Manchester University, as well as other investigators. This is a phase of the range of combustibility previously mentioned. The subject is one, however, that could be taken up further to good advantage by some university laboratory. The results should be of value, as should the research training afforded research assistants working on such a problem.

The rate of combustion in alcohol mixtures is said to be much slower than in gasoline mixtures. This has an important bearing on the design of alcohol engines. The evidence on this point is conflicting; moreover, the direct experimental results on ratio of flame speed under conditions comparable to those in an engine are few. It would be very much worthwhile for some laboratory interested in the subject of flame velocity to include alcohol mixtures in its study.

The general performance characteristics of engines using alcohol fuel have been studied to some extent, but mainly at full load. Cylinder-head design and spark-plug location are probably important. More power can be obtained sometimes from the same cylinder with alcohol than with gasoline, at the same compression, and a still greater excess at the higher compression-ratios permissible with alcohol. Part-load performance should be studied as regards thermal efficiencies and mixture requirements as well as general performance characteristics.

The incidental effects of alcohol on the engine involve one feature that is particularly important. This is corrosion of tanks, pipes and fittings. The metals now used for tanks, such as galvanized iron and tinner iron, are not suitable for use with alcohol and its continued use in these tanks will usually cause trouble. Some metals, such as aluminum and monel, are not seriously affected and could probably be used with entire success. Brass used in carbureters and fittings is not very seriously affected. There is also some possibility of corrosion in cylinders and mufflers, but the data on this are conflicting. The purity of the alcohol or its freedom from certain impurities probably determines the amount of corrosion. This whole question of corrosion is in need of very careful research, as it is perhaps the most important single hindrance to success in the use of alcohol fuel.

Complete answers to the above-stated questions should permit the design of engines that would use alcohol fuel with the same degree of satisfaction as is now had with gasoline. The foregoing discussion has applied to alcohol alone, but it would apply to blends of alcohol with other hydrocarbons. In fact, such blends have some decided advantages over either fuel alone. An ideal engine should be capable of using either gasoline or alcohol or any combination of them, and of realizing the inherent advantages of each with a minimum of modification necessary in changing from one fuel to another. Such engines should be very popular in countries where alcohol is cheap and gasoline dear.

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THE WASHINGTON CONFERENCE

THE jeopardy of war has been most assuredly driven beyond our generation. Naval rivalry with England has been stopped, with all the implications which must follow from that rivalry. The Anglo-Japanese Alliance has been abandoned. The points of friction between ourselves and Japan have been settled. The major difficulties between Japan and China have been adjusted so that good will may replace hate in the Far East. The tide of foreign aggression on the frontiers of China has been turned back. The "open door" of commerce and industry of all nations in China has at last been made effective. With all these solutions, a vast growth of malign political forces has been uprooted, which, if unchecked, would have driven unalterably to war.

With half of Europe armed on a basis that, in equal ratio, would mean a standing army of 1,500,000 men in the United States, with twice this number of reserves, there is little hope of economic progress. The economic loss in the decrease of national production and in the effect such decrease has on national finance is one of the great difficulties of Europe. Yet there is a greater loss than all this, for economic life can thrive only in an atmosphere of peace.

The first effort of the world should be to reestablish its sadly lowered standards of living, recover employment for its workers, recover income to its farmers and reestablish the prosperity of its manufacturers and merchants. The world is now, and will be for many years, short of capital with which to carry on the development of industry and commerce. America is today the remaining great reservoir of surplus capital, and we must remain strong and conserve the rightful use of this capital if the world is to recover. We shall need much of it for the upbuilding of our homes and our public utilities, stagnated today through inequalities in readjustment between industries. But our surplus even then is greater than our needs, and it would be a disaster if it were dissipated in wasteful expenditures abroad.

All loans to foreign nations that are not employed in reproductive work are a destruction of the capital. It is vital that our bankers and our investors should scrutinize the loans to which they are asked to subscribe, that they may make sure that they are to be employed for reproductive purposes or the refunding of outstanding obligations. The furnishing of raw materials, the construction of transportation facilities, public utilities and factories and work throughout the world, is a use for American capital that blesses both the borrower and the lender. The upbuilding of the rest of the world and its consuming power adds primarily to world wellbeing, but it also adds to the demands for our own labor, for the products of our own farmers and the services of our own merchants.—Herbert Hoover.

Some Notes on the California Top

By P. W. Steinbeck[1]

Annual Meeting Paper *Illustrated with* Photograph and Drawing

THE California top is the result of a demand for a convertible enclosure that combines the desirable features of the open top with the protection needed at night or in bad weather. The history of its development is outlined and shows the evolution of a top that can be stowed or removed quickly and that has a permanent roof and glass windows. The present California top and its construction is then described and illustrated.

THE California top represents the present-day culmination of a concerted effort on the part of body engineers in this Country to produce a really satisfactory type of convertible enclosure. This particular style took its name from the Pacific State since the development of a readily convertible open car was particularly demanded in California because of weather conditions peculiar to that region. The days are generally very warm there and demand the protection of an open top with ventilation unhampered by windows. The evenings, on the other hand, are accompanied by heavy dews and a relatively large drop in the temperature which makes one uncomfortable in an open car. The car-owners seldom wished to lower their tops, disliked the saggy appearance of the folding style and demanded something more pleasing to the eye. The body-builders and designers recognized this demand for a more durable and satisfactory type of curtain and top than the customary folding type with celluloid lights, and began the development of what has come to be known as the California top. An example of the present type of top applied to a touring car is illustrated in Fig. 1.

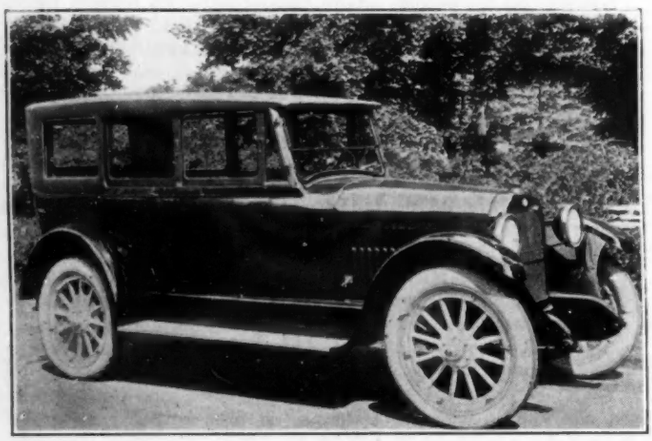

Fig. 1—Application of the California Top to a Touring Car

Development History

The first step, taken 6 or 7 years ago, was to construct a solid frame of wood and cover it with artificial leather, using glass lights in the sides and rear instead of the customary celluloid. This top did not represent any great improvement over the one-man top. It was more rigid, to be sure, but the bothersome flapping storm-curtains were retained and were attached loosely by fasteners or buttons. Nevertheless, these tops sold well. They were something different and, since most of them were custom-built, each particular owner had an opportunity to express his own fastidious tastes in form or color. The natural result of this self-expression was a conglomeration of designs and clashing color contrasts. The beveled-edged plate-glass windows in the back and rear-quarter curtains were fantastic in shape and represented anything from a half-moon to a diamond or oval.

A few years later a top appeared that represented the next stage in the creation of the present-day California top. Its outline, shape and trimming closely resembled the type just described, but the curtains slipped or rolled up into the space between the top deck and the headlining. They were concealed in this way when not in use and made a fairly weather-tight enclosuse when pulled down. There were several serious faults in this design, however. The curtains carried moisture into the top when rolled up after a rain-storm, causing the headlining to become moldy and to rot. The celluloid lights were dried out due to the heat in the top; they crystallized, became brittle and were soon fogged so that vision through them was impossible. Although this type proved to be popular for a time, the body engineers soon recognized it as a mere stepping-stone to the construction of something possessing the same utility but of greater durability.

A body-builder named Gould exhibited a closer approach to the ideal convertible top at the San Francisco Automobile Show in 1919. It was meant to fulfill the Californian demand for a top with permanent roof and glass windows that could be stowed or removed quickly. In this design the windows slid back into a pocket formed in the rear-quarter window, and were concealed in this way when out of use. Fig. 2 shows the features of this design, but with the details somewhat improved over the original top built and displayed by Mr. Gould.

Present Practice

This top can be built on any touring car or phaeton open body. A set of sills is first screwed securely to the body top-rail and, from this foundation, a slatted roof construction is built up over cross-bows, the slats being 1½ in. in width and running lengthwise of the roof. In the roof there are two U-channels with a drop as shown in Fig. 2. Each glass frame has a lock fastened to the top of the frame, which slides in a U-channel and, as each frame is suspended in the U-channel, they are pushed forward until they drop in the U-channel depression. The glass frame then drops into its position and becomes rigid. There is a separator on the sill that holds the frames apart. The space between the glass frames has two rubber strips to keep out the weather. Each glass frame can be pulled forward, or to the rear quarters, by attached handles. When the glass frames are in the rear quarter, they are held by a spring that presses down

[1] M.S.A.E.—Body engineer, H. H. Babcock Co., Watertown, N. Y.

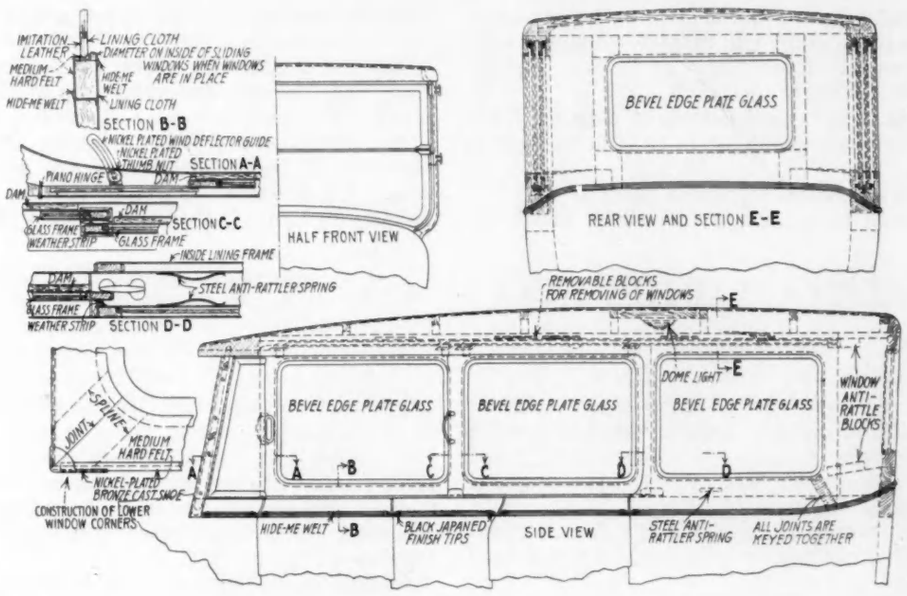

FIG. 2—DETAILS OF THE CONSTRUCTION OF THE CALIFORNIA TOP

from the top and prevents any rattle. The top is covered with the usual artificial leather or top material of the color desired and the inside is trimmed with cloth or slip-lining material in a manner similar to that used for closed-car roofs. The window frames are covered in the same way. Plate glass having beveled edges is used in the glass frames. When space permits, a glass-frame wind-deflector is hinged to the windshield which can be opened or closed by a quadrant and screw. A dome light equipped with a switch is installed in the roof. This type of California top is a distinct innovation in that it is a summer and winter top combined. It could be developed on a production basis and reduced in price to fit in and sell between the sedan and the touring-car types.

THE DISCUSSION

W. C. KARNS:—What provision is made on the bottom of the frames to keep the rain from blowing in?

P. W. STEINBECK:—There are two pieces of rubber fastened to the glass frames, that close the opening between the frames; on the bottom or sill there is a weather-strip on the inside of the frame that makes a water-tight job.

MR. KARNS:—Do those glass sides open with the door or must one slide the glass?

MR. STEINBECK:—One must slide the glass. If one is inside the car, the first movement in getting out of the door would be to push the glass back.

MR. KARNS:—I should think that would be objectionable.

MR. STEINBECK:—With a front and rear door and a space between them, as shown in Fig. 2 of my paper, only two windows are required to fill that space. Therefore, the windows must overlap the space between the two doors. Hence, the windows cannot be of the same size as the opening of the doors. It is a very easy matter to pull the glass frame back and get out. It moves in either direction. In driving, there are times when there is a strong wind blowing from one side. The driver is protected by the windshield and the deflector, but the occupants in the rear seat get the whole force of it. Even on a nice day with the wind blowing hard, they are uncomfortable; if one window frame is slid into position, it will protect the rear passengers.

MR. KARNS:—I think that is an objectionable feature.

MR. STEINBECK:—It is not so objectionable as trying to unbutton or undo a fastener when it sticks, perhaps pulling the whole fastener off. Compared to entering a car with curtained sides, when the curtains are buttoned down tight, and especially after they have shrunk, I think the improvement would be appreciated.

G. W. KERR:—How does the glass on the front door open?

MR. STEINBECK:—It slides on a channel. One glass hangs on one channel and the other glass on another channel. They slide past each other. The handles are flush.

MR. KERR:—How is the car ventilated?

MR. STEINBECK:—Through the wind deflector and by opening the windows.

CHAIRMAN E. G. BUDD:—Is the rail that is used fixed independently into each door?

MR. STEINBECK:—Yes, but it is screwed solid to the body to get a straight sliding space. We take an ordinary phaeton. We must have a certain space for the windows to slide in a straight line. If one started to design a body, it could be arranged to have the line straight and do away with the rail. When the glass frames are in the rear quarter position one cannot see the individual windows because the glass appears to be one window; this is, when all glass frames are pushed back, making an open car. There are other ways of accomplishing this, such as sliding on rods. I saw a design at the automobile show, by a Mr. Leach. He has only a one-quarter sliding glass frame and cannot go to the other quarter because he uses a curtain on a roller, fastened above the door. It is as tight as a sedan and he claims that it does not rattle.

MR. KERR:—What is the approximate cost?

MR. STEINBECK: — Individually, about $650, starting from the beginning. For instance, if one had an order for 200 or 300 touring cars to be built from the beginning, the tops probably could be manufactured for about $250, or less. On a single order one would find that all phaetons are not alike. Some have sides longer than others and some have more bevel; that is, one cannot fit

this top to one body, take it off and fit it to any other body. It must be made right and fitted to whatever body is under consideration. Each top must be laid out specially and be custom-built.

R. A. LA BARRE:—What advantage is claimed for a top of this kind over a regular sedan if it is built from the beginning?

MR. STEINBECK:—One has a regular phaeton with unobstructed view, and also a closed car, which meets the climatic changes of California and other places where it is warm during the day and chilly at night.

MR. LA BARRE:—It seems to me that construction of that kind, building from the body up, would be as expensive as a sedan body.

MR. STEINBECK:—No, not quite, as less and cheaper material is used. These tops are generally lined with slip-lining material, the labor cost is less and they can be manufactured in quantity for less than the sedan.

PHOTOGRAPHIC RECORDING OF ENGINE DATA

(*Concluded from page* 356)

more important than sensitivity. It is easier to limit oneself to periodic vibrations heavily damped and to obtain extra sensitivity from some other source. One can get all the sensitivity one desires; the main thing is to watch the damping effect.

DR. RICHARD H. CUNNINGHAM:—Will Professor Trowbridge explain the air damping that apparently is used in this instrument?

PROFESSOR TROWBRIDGE:—It is electro-magnetic. The air damping can be disregarded in this instance, as it is very small.

DOCTOR CUNNINGHAM:—I have been using the string galvanometer for many years in the investigation of various phenomena. I have used many of these short metallic strings, but have found that it is utterly impossible to use them where one must depend upon air damping. I was unable to get proper damping except by using short-period quartz fibers. Since about 1905 I have thought that we could use this apparatus in various forms to great advantage for the investigation of many of the phenomena occurring in the automotive industry.

Another instrument particularly applicable to electrical or ignition problems is the string electrometer. It can be made rather sensitive for very high periodic vibrations and will indicate faithfully many of the variations in vibration that we encounter. I first tried to use metallic strings in that apparatus, but really was unable to accomplish anything until I used very small quartz fibers, 1.0 to 1.5 microms (0.00003937 to 0.00005905 in.) in diameter, which possess practically no weight at all. In fact, they behave very peculiarly. If one is held by the fingers and released it usually goes up in the air instead of to the ground, just contrary to what one would expect. One of the points not brought out by Professor Trowbridge relates to the control curves obtained with these metallic strings and the use of proper resistance to get a definite electro-magnetic damping. What is the resistance of the circuit?

PROFESSOR TROWBRIDGE:—It is utterly out of the question to use quartz-fiber strings and that sort of thing for a practical working instrument. The history of this particular instrument is that, for Army use, I had to have something absolutely and completely foolproof and would work 24 hr. per day in the hands of men in dugouts and the ruined houses in France. At that time I did not need a highly damped and well-behaved instrument; since then I have taken up this question of damping and now the instrument is perfectly damped. It can be damped by shortening the string so that, if it moves, heavy currents will flow in the string; that is, it acts like a dynamo and, when buzzing, it drives the current around and therefore checks itself. One must use a low external resistance and adjust it until the system is practically periodic. Fortunately it has so much sensitivity for purposes of this sort that one can afford to throw away 90 per cent of the sensitivity to secure the complete impartiality of its motion; that is, it has no properties of its own whatsoever, not the slightest. Whatever is recorded is due to something that has been impressed upon it. I have a record of an oscillating circuit obtained by breaking an inductive circuit, and its form is exactly what the theory demands.

THE COTTON INDUSTRY

FROM 1870 to 1910 the general trend of world cotton production and consumption was very steady, manifesting a tendency to increase progressively. During this period the per capita consumption of cotton the world over increased about three and one-half times. During the decade beginning with 1910 fluctuations of the most violent sort became common. During the past 10 years variations of from 2,000,000 to 4,000,000 bales occurred on five different occasions.

On the basis of the world's consumption of cotton in the past, it would seem fairly reasonable to assume that consumption for the next few years should be in the neighborhood of 19,000,000 bales. With the exceptionally large stocks eliminated and the prospect of a fairly steady demand, there apparently exists no fundamental factor that should bring about any violent fluctuation in the price of cotton in the near future other than those resulting from crop conditions. The world's population has undergone no material change in recent years, and new uses for cotton are continually being found. It should be fairly safe to predict that as conditions the world over improve economically and politically the dominant influence affecting cotton should be that of a steady increase in the demand for the staple both at home and abroad.

Exports from the United States should be considered from the standpoint of both volume and value, although it is found that there is a practically fixed proportion of the country's crop exported over five-year periods. This proportion is placed at 66 per cent of the crops.

It would seem that cotton is in a stronger position fundamentally than it has been in some time, and that as a consequence its price should be fairly stable in the near future. The above conclusion is based upon the assumption that there will be no material change one way or the other in the production of cotton from the volume and tendency that were manifest in prewar years. Any arbitrary change in the acreage planted would, of course, be a factor that would have to be given due consideration.—W. F. Gephart, vice-president and director of industrial service department, First National Bank in St. Louis.

Discussion of Papers at the Annual Meeting

THE discussion of the papers presented at the recent Annual Meeting of the Society included written contributions submitted by members who were unable to be present and the remarks made at the meeting. In every case an effort has been made to have the authors of the several papers reply to the discussion, both oral and written, and these comments, where received, are included in the discussions. For the convenience of the members, a brief abstract of each paper precedes the discussion, with a reference to the issue of THE JOURNAL in which the paper appeared, so that members who desire to refer to the complete text as originally printed and the illustrations that appeared in connection therewith can do so with a minimum amount of effort.

In addition to discussion given below the discussion of Economics of Motor Transport, by M. C. Horine; Recent Research Work on Internal-Combustion Engines, by Harry R. Ricardo; The California Top, by P. W. Steinbeck; and Photographic Recording of Engine Data, by Prof. Augustus Trowbridge, is printed elsewhere in this issue of THE JOURNAL, following the individual papers, The discussion following the presentation of the paper by Prof. R. E. Wilson and D. P. Barnard, 4th, entitled the Mechanism of Lubrication, will, it is expected, appear in an early issue of THE JOURNAL, together with the paper.

VISCOSITY AND FRICTION

BY WINSLOW H. HERSCHEL

THE author divides the study of lubrication into different regimes in which different properties of the lubricant have the controlling influence and discusses at length viscosity effect in the complete-film-lubrication regime, including comment on the properties of lubricants, units for viscosity measurement, absolute viscosity relation to the readings of instruments ordinarily used and mathematical analysis, with reference to data that are presented in tabular and chart form.

Viscosity estimation at one temperature from the observed viscosity at another temperature is then treated in a somewhat similar manner, inclusive of discussion bearing on complete film lubrication, as are the subjects of journal friction and viscosity in which the mathematical analysis is continued, and friction tests are described briefly, copious specific references being given.

Consideration is given to the transition point, the relation between the different factors at the transition point and incomplete film lubrication and the oiliness of lubricating oils, the last including a description of Bureau of Standards tests.

A lengthy discussion of the differences in oiliness of different lubricants shown by laboratory and service tests follows, many references to tests and testing machines being cited and quotations made from reports.

Ten specific desirable features of oil-friction testing-machines are enumerated and commented upon in the light of previous experiences that are cited, and nine specific conclusions regarding viscosity and friction are stated.—[Printed in the January, 1922, issue of THE JOURNAL.]

THE DISCUSSION

H. S. MCDEWELL:—In connection with the maximum temperature at which the viscosity is desirable and in regard to some of my own work, I had occasion to determine approximately the temperature of the oil-film existing in an Hispano-Suiza Model-I aeronautical engine. The cylinder construction of that engine is such that the heat is transmitted through two metals, a steel cylinder and the aluminum body into which the steel liner is screwed. No matter how carefully the machine work may be done, there is an infinitesimal gap between the two metals. Consequently, the temperature existing inside the cylinder, for a given cooling-medium or jacket-water temperature, is greater than in an engine in which the construction is such that the heat must be transmitted through only one metal. In the comparative test of two oils of the same base but of material difference in viscosity, the oil consumptions were compared by a cut-and-try process on the basis of the viscosity existing at an assumed oil-film temperature. On that basis it was found that the oil consumptions were exactly proportional to the respective fluidities at 210 deg. fahr., which are the inverse of the viscosities, thus giving a probable temperature of oil-film in the neighborhood of 210 deg. fahr. for a Model-I Hispano-Suiza engine. In an engine of conventional cylinder construction using only one metal the oil-film temperature is probably below this figure; consequently, Dr. Herschel's remark in regard to the inadvisability of the determination at 350 deg. fahr. is substantiated. The reason that the temperature mentioned is so low probably is that in heat transmission there is always a dead-gas film between the source of heat and the metal. In this film the temperature drop is very great. The dead-gas film in this case exists outside the oil-film and consequently serves to protect it. There is no question that, if the oil-film were subjected to the temperatures actually existing during combustion, there would be no oil left on the cylinder walls. It would certainly be burned up.

T. E. COLEMAN:—Is there a connection between viscosity and friction, and the clearance of a piston or a bearing? That might come in with reference to oiliness, I imagine. I have in mind particularly a car guaranteed to run 15,000 miles without oil-pumping. The salesmen inform me that the aluminum piston used has less than 0.002 in. clearance when cold and that means of compensating for the expansion is provided. That piston must expand somewhat and I think there must be some connection between the kind of oil used and the thickness of the oil-film.

DR. W. H. HERSCHEL:—According to the ordinary theory or definition of viscosity, the resistance is inversely proportional to the thickness of the oil-film; that

is, the narrower the clearance, the greater the friction. Therefore, by reducing the clearance to a very small value, one would increase the friction, in complete film lubrication; but, in all probability, there is seldom if ever complete film lubrication in the cylinders. It exists generally in a journal bearing in which forced-feed lubrication is employed, but in cylinder lubrication, in which oiliness rather than viscosity is the predominant factor, it is very difficult to answer the question accurately because there is a condition of incomplete film lubrication in the cylinder. We are subjected to laws of lubrication that include this factor of oiliness, which is rather vague and indefinite, and we cannot say with certainty just what the effect of the variation of clearance would be.

DR. GEORGE W. COGGESHALL:—What is Dr. Herschel's opinion as to what constitutes oiliness?

DR. HERSCHEL:—Possibly to a chemist this paragraph from a review of a paper by W. B. Hardy will throw some light on the subject.[1]

> The relation between lubricating power and chemical constitution was investigated by measuring the force required to move a slider of bismuth over a surface of the metal. The static friction in a simple chemical series, such as the fatty acids, the alcohols and the paraffins, decreases with the molecular weight but not in a simple manner, the chemical constitution playing an important part. At the point in the series where the lubricant becomes a solid at the temperature of observation, there is no break. The more saturated cylic compounds are better lubricants than the less saturated cylic compounds, but no ring compound is a good lubricant. As lubricants esters are inferior to their related free acids and alcohols, but to this rule the ring esters are exceptions. The hydroxy acids have remarkable lubricating power, but the ring compounds are again exceptional.

Oiliness, as I defined it, is the factor or factors due to chemical composition or anything else that will cause a difference in friction when two lubricants are used in the same machine under identical circumstances. For example, with a cutting oil one has very high concentrated pressure within a small area.[2] This brings out the effect of oiliness. Everyone knows that a lard oil is a good cutting oil. If a mineral oil of the same viscosity as lard oil is used, one gets a comparison of the effect of oiliness because, both being of the same viscosity, the effect of viscosity is excluded, although I will admit that there is a slight influence from the change of viscosity with the temperature. We know very little about the temperatures of the oil-film in cutting metal. We have tried cutting off discs from a steel bar with different oils. The tool was fed mechanically so that the conditions of use were constant. With one oil we have perfectly smooth discs that look as if they had been polished; with another oil we have jagged surfaces. That difference, so far as I can make out, is due to the difference in oiliness.

DR. COGGESHALL:—Is there some chemical difference?

DR. HERSCHEL:—The difference is perhaps in that intermediate region in which it is difficult to determine whether it is chemical or physical. Probably it is due to what is called adsorption or adhesion. It is the interfacial tension between the metal and the lubricant. It is the force with which the lubricant adheres.

DR. COGGESHALL:—Would the quality of the metal have an effect?

DR. HERSCHEL:—The quality of the metal certainly does have an effect. There is an enormous difference in friction when one uses the same lubricant on different metals. In other words, if there is an inter-action between a lubricant and a metal, this inter-action will be affected both by the quality of the metal and by the quality of the lubricant; it is this inter-action between the two that makes the oil-film adhere to the metal and determine the coefficient of friction.

J. W. STACK:—Has Dr. Herschel made experiments in a comparative way between the mineral and the fatty oils as to surface tension or liquid to solid angle?

DR. HERSCHEL:—Surface tension is best explained in the work of Wells and Southcombe.[3] They find that surface tension as given in the ordinary tables has no effect, because the surface tension of all oils is practically the same; but, on the other hand, if one measures what is called inter-facial tension by the size of a drop forming under water, a method developed by Donnan but used by Wells and Southcombe, one will find great differences between oils. It is this inter-facial tension, and not surface tension, correctly speaking, that makes the difference or at least is an indication of the difference in oiliness.

We are now investigating the effect of variation in the organic acidity of mineral oils due to causes one cannot explain but not due to compounding with fatty oils. We are trying to determine the effect of changes in this organic acidity due to petroleum acids, on inter-facial tension and, hence, upon oiliness and the coefficient of friction.

R. W. A. BREWER:—The author refers to oil-films "of the order of 0.000125 in." It seems to me that the errors in smoothness of surface due to its mechanical formation are so very great in comparison with these very small dimensions that it is almost futile to talk commercially of oil-films of that thickness.

DR. HERSCHEL:—The information concerning the thickness of oil-films is very meager. Very little work has been done on the subject. In very few cases has the film thickness been measured and yet, as the film thickness is one of the factors that comes into all equations for determining the turning moment or coefficient of friction or the relation between viscosity and friction, it cannot be neglected. One cannot check the equation for determining the relation between viscosity and the coefficient of friction unless one knows the film thickness.

MR. BREWER:—Do not those film thicknesses come into play at very much larger dimensions than these that are given? The point I raise is that the film dimensions are so very minute. They are much smaller than one meets in the ordinary practice of lubrication.

DR. HERSCHEL:—Possibly Mr. Brewer has overlooked the fact that the thickness of oil-film that he cited was in gear lubrication, and that in journal bearings the journal is not concentric in the bearing except at very high speeds. At very high speeds it approaches the concentric condition, but at moderate speeds the journal is eccentric, and therefore the film thickness at its thinnest place is less than one-half the difference between the radii of the journal and the bearing. With metallic contact, as in the case of rest, it is obvious that there will be practically no film thickness at all, or at least nothing but a film thickness due to the adsorbed film. Therefore, one does meet in practice, and certainly when approaching the state of rest, film thicknesses that are as small or smaller than the values I gave.

MR. BREWER:—These are only local thicknesses, I take it, at the highest spots of the two surfaces.

DR. HERSCHEL:—They are at the point of nearest ap-

[1] See *Chemical Abstracts*, vol. 15, p. 3159; for original see *Proceedings of the Royal Institute of Great Britain*, Feb. 27, 1920.

[2] See Bureau of Standards Technologic Paper No. 204.

[3] See *Journal of the Society of Chemical Industry*, vol. 39, p. 51T.

proach. The condition is not due to the irregularities in the surface, but to the fact that the journal is eccentric in the bearing.

MR. BREWER:—Are the thicknesses of oil-film measured from the parts of the metal that are in closest contact?

DR. HERSCHEL:—Yes, at the point of nearest approach.

NEIL MACCOULL:—Professor Kingsbury made some interesting experiments several years ago to determine what constitutes "oiliness." When I asked him whether he found much difference between oils as far as oiliness is concerned, he asked me the pertinent question, "What is oiliness?" Mr. Deeley has developed a machine in England for measuring oiliness; it is apparently a machine for measuring the static coefficient of friction. However, that is not the whole story because, when the machine is very heavily loaded, all kinds of disturbance are evident with an ordinary mineral oil; but with a fatty oil the machine runs much more smoothly. Prof. R. C. Carpenter, of Cornell University, made experiments many years ago in which he tried to measure the wear of different bearing-metal alloys. His machine was devised so that he was able to measure an appreciable amount of wear after a few hours of running. The interesting fact was demonstrated by him that when a series of alloys having different percentages of antimony and lead was investigated, the minimum wear was with lead, which gave the maximum friction, and the alloy showing the minimum coefficient of friction showed the maximum wear. Can we expect that, if one oil is more oily than another, the wear that develops in a bearing will be less with the oil that is most oily? It may be that the matter of wear does not vary in the same direction as does friction.

CHAIRMAN H. C. MOUGEY:—Is there any difference in the oiliness of mineral oils, or is it necessary to add a second oil to the mineral oil to obtain this property of oiliness?

DR. HERSCHEL:—In considering what Mr. MacCoull has said, one should refer to the portion of my paper on the friction testing of bearing metals. Primarily I am not a metallurgist; I am an oil specialist and have not emphasized this subject of testing bearing metals because I am interested in lubricants. In my paper concerning the testing of bearing metals I have quoted Goodman's opinion, that all wear in friction tests is due to the imperfect fitting of the bearing and the journal. Therefore, I think we must accept with caution any conclusions reached in regard to the comparative amount of wear. If Goodman is correct, the wear observed is merely an indication of variation in fit between the bearing and the journal, and not an indication of anything else.

Chairman Mougey's question regarding the necessity for adding fatty oils to improve the oiliness opens up a most interesting subject; that is, whether organic acidity due to a petroleum acid in the oil, and not to compounding, is beneficial in the same way as the organic acidity due to the compounding or addition of fatty oils. The subject is under investigation. There is also the question whether, by an addition of graphite in the colloidal form so that it will not settle out, we may obtain the same advantage in reducing the coefficient of friction that is gained by the addition of a small percentage of fatty acid, as found by Wells and Southcombe.

CHAIRMAN MOUGEY:—If I understand correctly, there would be no difference in the oiliness of mineral oils except for the presence of foreign matter, possibly a petroleum acid of some kind, that is present in one mineral oil and not in another. Does that mean that the more highly refined oils would be inferior as lubricants because these desirable qualities have been removed during the process of refining?

DR. HERSCHEL:—It depends entirely upon what is meant by the term "foreign matter." Until we know the source of the acid in the test for organic acidity of a pure uncompounded mineral oil, we shall not know whether to classify oils of perceptible organic acidity as containing foreign matter.

There are several possibilities as to the sources of petroleum acids in oils. They may be in the original crude; they may be a result, let us say, of cracking in the distillation; they may be a result of treatment with sulphuric acid. If there are certain hydrocarbons in the oil as a result of any of these processes of refining, it is a matter of very exact definition to determine whether these recently formed hydrocarbons should be called "foreign matter." They perhaps are not present in the original petroleum, but possibly are oxidation products of the hydrocarbons in it. Do they or do they not constitute "foreign matter?" We do not know as yet whether these petroleum acids are an advantage or a disadvantage.

There may be such a thing as over-refining and removing too great an amount of these acids. From that viewpoint it is just as bad to over-refine an oil as not to refine it enough. We lack methods of testing to indicate when an oil is refined enough and when it is refined too much. We need a method of testing to indicate when an oil has been sufficiently refined. It seems probable that it is a matter of compromise. Apparently, we want enough of these unsaturated products in an oil to give good oiliness. If we have too many unsaturated compounds in the oil, it will have a tendency to gum. The new school of thought seems to indicate that to give good lubrication we should have an oil that will gum. Quoting from R. M. Deeley:[4]

> It appears then that the true lubricant is an unsaturated compound possessing all the attributes of such a compound; that is, (*a*) capacity to absorb iodin, bromin, oxygen and so on; (*b*) solubility in strong sulphuric acid; and (*c*) a higher carbon-hydrogen ratio than the saturated derivative.

That sounds very much as if the author were defining a drying oil. According to that definition we would expect linseed oil to be the best substance for a lubricant, but common sense tells us this is not true. Therefore, if there is any truth in this, we must say that it is a matter of compromise between enough of the unsaturated compound to give good lubrication and not enough to cause gumming.

DR. COGGESHALL:—It is known to be true, I believe, that the addition of fatty or organic acids to mineral oil will increase the oiliness. Have those increases always been determined on bearing metals subject to attack by an organic acid? There are a few metals that are not attackable by organic acids. Have any experiments been made using those metals as journal bearings and as shafts? If a platinum journal were used, we could assume, I think, that there could be no chemical action between the platinum and the organic acids. We do know that organic acids will attack iron and other ferrous metals. If the addition of organic acids to mineral oils did not increase the oiliness on a platinum journal, we could pretty nearly conclude that there was no chemical combination between the fatty acids and the journal. If the addition of organic acids did increase the oiliness,

[4] See *Proceedings of the Physical Society of London*, vol. 32, p. 48.

we would be forced to conclude that there was some chemical change in the lubricant, rather than a combination with the metal of the journal. There might be adsorption on platinum metals just as on ferrous metals.

DR. HERSCHEL:—It seems to be a question whether to include adsorption with chemical or with physical action. This I will not undertake to decide. I think it is safe to leave it that way: a case of adsorption. The report of the Lubricants and Lubrication Inquiry Committee explains the work done in England. This committee not only obtained the results directly by friction tests but has used the inter-facial-tension apparatus. There is a slight hiatus in the line of argument, because it is impossible to detect directly the attraction, inter-facial action, the adsorption or whatever one wishes to call it, between a lubricant and a bearing metal. It must be determined by indirect means, by the inter-facial action between a lubricant and water, but the conclusions derived from the observation of drops of oil forming under water have been substantiated by tests made on friction-testing machines.

B. F. TILLSON:—As I understood him, Dr. Herschel referred to the saturated hydrocarbons as being the lubricating bodies and later spoke of the unsaturated hydrocarbons as contributing the lubricating qualities to an oil. This confuses me.

DR. HERSCHEL:—It should be "unsaturated hydrocarbons," according to the modern theory. These are supposed to be the valuable constituents of the oil as far as lubrication is concerned.

MR. TILLSON:—That is what I have heard from other sources. In that respect it is of interest to note some tests made of oils distilled from oil-bearing shales. Some of these oils show very light viscosities and low densities. Their lubricating qualities under test conditions thus far have seemed to be superior to those of the average internal-combustion-engine lubricant. When tested in passenger cars and trucks, greater mileage per pint of oil consumption was shown, with an apparently greater engine horsepower in the same engine. These oils are from a petroleum-base shale-oil. In the process of distillation, possibly due to cracking in the retorting of the shale, they probably contain a large percentage of complex unsaturated hydrocarbons.

RELATION BETWEEN FLUID FRICTION AND TRANSMISSION EFFICIENCY

BY NEIL MACCOULL

THAT all of the variable factors of automobile friction-losses such as the quantity and viscosity of lubricants, the efficiency of worm-gearing and part-load modifications are not appreciated, is indicated by an examination of the literature on this subject which reveals a lack of necessary data. Experiments to determine the mechanical losses, including all friction losses between the working gases in the engine and the driving-wheels of the vehicle, are described and supplementary data are included from Professor Lockwood's experiments at Yale.

Three distinct possibilities for increasing the fuel economy of a motor vehicle are specified and enlarged upon, gearset experiments to secure and develop data for a four-speed gearset being then described and commented upon at length; photographs and charts illustrative of the equipment used and the resultant data are included. The effects of varying the speed under no-load and load conditions are studied, inclusive of mathematical analysis, and the efficiency of the gearset and noise measurements made in regard to it are discussed. The paper is then summarized. [Printed in the March, 1922, issue of THE JOURNAL.]

THE DISCUSSION

R. W. A. BREWER:—The Hopkinson method of carrying out a test of this sort is interesting. Some 30 years ago Dr. Hopkinson developed a system of testing alternating-current generators that has been modified in this particular test. It seems to me that this method is adaptable to any kind of rotary machine and should be adopted very much more widely. I am very glad to see that it was used in this case, and also that results were obtained which can be relied upon.

Fig. 1 of the paper refers to the mechanical losses and to wind resistance and tire losses. Mr. MacCoull thinks that not much work or investigation has been done along these lines. But much has been done. Between 1909 and 1914 I was doing considerable automobile racing on the Brooklands track. In the course of the work it was necessary to design cars to give a guaranteed performance. To do so wind and other resistances had to be considered, because these resistances are the principal factors in the whole design. To determine the accuracy of the design some tests were made on a single-seat racing-body, placing an ordinary number-plate on the front of this body, and subsequently a four-seat automobile, a sporting model, was designed having a difference of resistance under test conditions exactly equal to the resistance of that number-plate. Tests of this kind show that designs for practically any predetermined wind-resistance can be made. Tests of that particular body show that one can make automobile-body forms to give an extremely low resistance if the proper method is used.

Mention was made of variations in resistance in a gearbox with varying quantities of oil. It is an old racing trick to put practically no oil in the transmission; such oil as does go in is, as a rule, engine oil. That makes a considerable difference in the friction loss.

Two points of resistance are very important in an automobile. One is the propeller-shaft loss, which varies with the design of the propeller-shaft and of the universal-joints. The other loss is due to faulty springing and to wheel-spinning. It is obvious that wheel-spin is a very prolific source of lost energy in the propulsion of a car.

PROF. E. H. LOCKWOOD:—A novel experiment on the measurement of wind resistance of vehicles is now being carried out at the Kansas State Agricultural College, Manhattan, Kan. The general plan is to expose the vehicle in a suitable location to the sweep of the Kansas winds, which are said to be both intense and steady at times. The test vehicle is placed on a circular turntable floating on water in a concrete reservoir. In this way the vehicle can be oriented to face the wind, the direction and velocity of which are indicated by the usual instruments. The total force of the wind exerted on the vehicle is measured by the dynamometer pull on the frictionless

floating turntable. Interesting and valuable results may be expected from this equipment.

Mr. MacCoull's experiments showing a marked increase of gearbox friction at low temperatures have been confirmed by dynamometer tests at the Mason Laboratory, Yale University. In the latter experiments the procedure was modified by chilling the entire transmission to about 32 deg. fahr. by allowing the car to stand overnight in the open air. The entire chassis friction, both front and rear, was measured in the cold state, and at intervals of 30 min. thereafter. The gears and transmission were run under load to warm-up the lubricant, until the latter reached a temperature of about 130 deg. fahr. Two typical examples are given to show the extent of friction variation produced in this way.

Quartermaster Standard Class-B truck No. 432,799 had run 3600 miles at the time of test and was in excellent condition. The empty truck weighed 11,540 lb., of which 6875 lb. was on the rear wheels. Rolling-resistance measurements were made with gearbox temperatures at 32 deg. fahr., these finally reaching about 130 deg. fahr. The cold test showed, for the front wheels, 80 lb.; for the rear wheels and the transmission, 166 lb.; or a total of 246 lb. The hot test showed 80 lb. for the front wheels; 130 lb. for the rear wheels and the transmission; or a total of 210 lb. The increase in the total rolling resistance due to chilling the lubricant was about 17 per cent.

A similar test was run with a duplicate Class-B truck, that had run only 300 miles, in which the engine and the transmission gave evidence of excessive internal friction. The rolling resistance of this truck was considerably greater. The cold test showed, for the front wheels, 83 lb.; for the rear wheels and the transmission, 295 lb.; or a total of 378 lb. The hot test showed 83 lb. for the front wheels; 190 lb. for the rear wheels and the transmission; or a total of 273 lb. The increase in the rolling resistance due to chilling the lubricant was 39 per cent. Intermediate measurements showed that the decrease in the friction was rapid as the lubricant warmed-up, but slow from temperatures of from 80 to 130 deg. fahr.

A MEMBER:—In Fig. 6 of the paper, do any of the curves shown refer to greases? Were the two transmission units that were put together statically balanced? If not, the ratings taken need to be corrected considerably.

NEIL MACCOULL:—In Fig. 6 the cross-arm is over the gearboxes, with the spring-scale by which we measured torque losses on the left-hand side. At the right-hand side of the gearboxes large iron weights are shown suspended from the end; these were installed to balance the gearboxes. All our readings were corrected by about 1½ lb. to compensate for the lack of balance.

In regard to the use of greases I place little reliance on the results I have given, because it is so difficult to measure their temperatures. We have no assurance whatever that the temperature of the grease where the thermometer is inserted is the same as the temperature of the grease around the gears. However, the curve for oil No. 3, as it is called in Fig. 9, is a grease that was tried, but I question that also to some extent because, as shown in the cross-section of the gearboxes in Fig. 4, there is a little pocket at the left-hand end of the gearbox where the engine shaft goes through between two of the roller bearings. It is very clear that after operating for some time this space would be filled with the lubricants used. The tests we made were of too short duration to make certain that the grease got up in there; so, it may be that the losses would be higher after a long test. Oil No. 7 was a light mineral oil in which there was a small percentage of grease, but not enough to prevent it from being fluid. I complained about the difficulty of cleaning out a grease, having in mind a particular grease that would not flow of itself. It can be removed only by digging it out, since it is not soluble enough in kerosene or similar liquids to wash out easily.

H. C. DRAKE:—In his work on transmissions did Mr. MacCoull make any tests to determine the losses with the gears in neutral?

NEIL MACCOULL:—No, because one would get only the readings from one gearbox. The same thing is true with a gearbox on an idler. That would be interesting, of course; our experiments were very incomplete.

H. C. DICKINSON:—In discussing this very interesting and valuable paper I should like to call attention to one point that the author seems to have overlooked, and that may be of considerable importance. In using the two gearboxes mounted in a cradle so that the resultant torque on them alone can be measured, Mr. MacCoull adopted a method that is in accord with good experimental procedure; that is, he has succeeded in measuring directly the quantity that he wishes to measure instead of deducing the result as the difference between the power input and the power output. The latter method has the inherent disadvantage that both measurements must be made with extreme accuracy to give any reasonable accuracy to their difference.

But, in adopting this method, the apparent torque on the gearbox yoke system is dependent on its center of gravity. The grease or oil in the gearboxes is a part of the system and any displacement of it can change the position of the center of gravity and thus change the balance of the system; or, in other words, the measured torque independently of the actual torque applied. When the gears rotate, they displace the oil in the gearcase and change the center of gravity by an amount depending upon the shape of the gearcase, the viscosity of the oil, the depth of oil, the speed and direction of rotation and perhaps other things. Moreover, the system as used is not symmetrical as regards displacement of the oil; hence, there is no assurance that the displacement in one of the gearcases will balance that in the other. As a matter of fact, they will both tend to change in the same direction.

The magnitude of these corrections for a Class-B-truck transmission has been determined incidentally by S. von Ammon, of the Bureau of Standards, in connection with a study of efficiencies in rear-axle construction for the Quartermaster Corps. With this gearset, the horizontal displacement of the center of gravity necessitates corrections which are of the same order of magnitude as some of the total net torques measured by the author. While these errors may be much less in the gearcases he has used, it is hardly safe to neglect them without a careful determination of their magnitude. For instance, it is possible that the constant K, which the author states depends upon viscosity and oil level, is in reality not a characteristic of the gear and bearing friction at all, but a measure of the displacement of the center of gravity of the oil. At any rate it has the same general characteristics as the error introduced by displacement of the oil.

MR. MACCOULL:—The possible error due to the changing of the center of gravity when the gears displace oil is very difficult to calculate. Observations of the effect of gear rotation on the displacement of the oil show that the oil usually builds up around the upper shaft as a

symmetrical mass having about the same center of gravity as the shaft. The oil thus picked up and carried by the gears has the effect of lowering the level of the remaining oil, but no observations were made to determine the extent of this. There is little lateral change in the center of gravity. It does not seem possible that the variation in the height of the center of gravity could be as great as that caused by changing the quantity of oil present from 4 to 8 qt., which would introduce an error slightly in excess of 3 per cent. To eliminate this source of possible error, further tests will be made on the effect of changing the quantity of oil.

CONTINUOUS DIE ROLLING

BY G. R. NORTON

THE process of continuous die rolling and the products possible with this method of manufacture are described and illustrated. The improvements that have been made were the result of efforts to produce more complicated sections by this process, with greater accuracy, and these are discussed at some length.

The physical characteristics of steel that must be considered are commented upon and forming that is effected in one pass is described, consideration being given the requirements of rolled forging blanks.

The cost of operation is treated and the equipment used is discussed, showing how this process differs from other methods of making the same things, as to both the operations necessary and the character of the product. [Printed in the January, 1922, issue of THE JOURNAL.]

THE DISCUSSION

G. R. NORTON:—To make the process perfectly clear, I wish to point out wherein it differs from the other rolling processes that are somewhat similar. This consists of the manner of forming the variable-section bars which is done entirely in the final pass after the bar has been reduced from a billet through an ordinary roll train. This final forming operation can be made in the last stand of a set of rolls, or in a separate finishing stand, the latter being a little more flexible and having a better arrangement. The only articles that have ever been made commercially in this way are clip-sections, wagon-box straps and some things of that character. They have been produced for many years by the Carnegie Steel and other companies but usually take the form of round sections alternating with flats, half-rounds or half-ovals, and no attempt is made to put impressions in the pair of rolls; one roll carries an impression while the other is perfectly flat. In the process described in my paper we match the impressions in the rolls by using a set of gears opposite the driven end of the rolls. We can match the rolls exactly by a method of adjustment, roll symmetrical sections, make offsets and perform similar operations. The improvements have been entirely along these lines. There is nothing particularly novel about the idea; in fact, it was advanced in 1835. Except for the cases mentioned, I think very little, if anything, has been put on the market because of the difficulties of production. One limiting factor is the amount of reduction that can be obtained without producing a flash or over-fill on the bar. In the ordinary rolling process the over-fill cannot be taken care of, but in many pieces we regard that exactly the same as the forging flash produced in the drop forging.

J. H. NELSON:—The process is exceedingly interesting and, if carried far enough, would, I think, be exceedingly useful as a preliminary to drop forging. Just what can be done in regard to it depends upon whether it can be made to pay. That is the great problem, just as it is a problem now in forging automobile parts. It is necessary, as was stated, to forge down to distribute the material to the proper places. If sections could be made of the proper shapes, it would save much time in the forge shop and increase production. The effect, however, would be to require a stock of a particular size and character for each job. In other words, it would not be possible to stock up a yard with a supply of steel. It thus might work a hardship on the customer, particularly during times when only small orders are placed. On certain classes of orders requiring one exceptionally large number of either identical or very similar forgings such a procedure might work out very satisfactorily.

CHAIRMAN F. P. GILLIGAN:—What is the etching substance used on those sections?

MR. NORTON:—I think it is iodin.

METHODS OF MEASURING DETONATION IN ENGINES

BY THOMAS MIDGLEY, JR., AND T. A. BOYD

THE various methods employed to measure detonation or fuel knock in an internal-combustion engine, such as the listening indicator, temperature and bouncing-pin, are discussed and the reasons all but the last cannot be employed to give satisfactory indications of the detonation tendencies of fuels are given. The bouncing-pin method, which is a combination of the indicator developed by the author and the apparatus designed by Dr. H. C. Dickinson at the Bureau of Standards, is illustrated and described. In this method the evolution of gas from an electrolytic cell containing sulphuric acid and distilled water measures the bouncing-pin fluctuations in a given period of time. The accuracy of this method of comparison is brought out in a table.

The qualities that a standard fuel must possess are explained and the objections to a special gasoline are pointed out. The use of a combination of a non-detonating and a detonating fuel as the standard is developed and the reasons that led to the use of hexane with diethyl selenide and isopropyl nitrite for a fuel standard are mentioned. [Printed in the January, 1922, issue of THE JOURNAL.]

THE DISCUSSION

A. C. BOCKMANN:—In the matter of a standard of comparison for apparatus for measuring detonation, I gather that there would be some variation. How is that determined?

T. A. BOYD:—A standard fuel, the characteristics of which are known, is always used as a basis of compari-

son, on account of the condition to which Mr. Bockmann refers. By running the standard fuel at the same time and under the same conditions as the fuel under examination, the variables introduced by changing engine conditions and necessarily always present are ironed out.

THOMAS MIDGLEY, JR.:—The method adopted is that of measuring the fuel, not the engine. When applying the measurement to the engine, we can compare any engine with any other. By this method we can determine very accurately a blend of the standard fuels that would just verge on detonation in a given engine. Let us assume that it has a 40 to 60 value. We can then take the same mixtures to another engine and determine the value for that engine, obtaining, say, a 50 to 50 value. These terms would then express the measurements of the engines. Thus, measuring the engine gives us practically the same results concerning detonation as measuring the difference in fuel oils.

CHAIRMAN A. L. NELSON:—My understanding is that with two standard fuels we determine two points, an upper and a lower point after detonation. Are the intermediate points on a straight line?

MR. BOYD:—They very seldom lie exactly in a straight line, but usually in a smooth curve. As many concentrations of the standard with which comparison is being made can be employed as is necessary or advisable. A smooth curve, which never has a great deal of curvative, can then be drawn through these points; or, if a high degree of accuracy is desired, such concentrations of the standard may be used as lie so close to the fuel under examination in detonating characteristics that the points can be joined by a straight line without introducing any measurable error. Thus, for example, in the cases reported in the paper, in which the operator was endeavoring to determine the composition of unknown blends of kerosene and gasoline, he proceeded as follows: From the stock of the same ingredients he prepared by trial two blends, one of which had a very slightly greater detonating tendency than the fuel under examination, as measured by the amount of gas evolved, and one of which had a slightly less tendency to detonate. A number of runs were then made with each of the fuels, checking them against one another and averaging the results. The averages thus obtained from each of the fuels of known composition were plotted on a chart having the amount of gas evolved and the relative kerosene-gasoline composition as its vertical and horizontal axes, respectively, and the two points were joined by a straight line. Projecting down from the point at which this line crossed the horizontal line corresponding to the volume of gas evolved by the fuel under examination then gave its composition directly, and with a surprising degree of accuracy.

A MEMBER:—We have here a definite addition to the methods of ascertaining the nature of detonation. There have been many misgivings as to just what detonation is. Any means that will help determine the line of demarkation between detonation and highly intensive burning is of value. Perhaps some portions of Mr. Ricardo's paper concerned a near approach to detonation that was not detonation. It may be that after more work shall have been done upon the subject we shall be placed in possession of an instrument that will be capable of telling us exactly what occurs, and we shall be able to find some line of demarkation where detonation stops and highly intensive burning begins.

PERTINENT FACTS CONCERNING MALLEABLE-IRON CASTINGS

BY ENRIQUE TOUCEDA

ADDRESSING the structural engineer and the purchasing agent particularly, the author discusses the relationship between them and the foundryman with regard to malleable-iron castings and enumerates foundry difficulties. The characteristics necessitating adequate gating for such castings are described and illustrated, inclusive of considerations regarding pattern design, followed by a statement of the considerations that should influence the purchasing agent when dealing with foundrymen.

Possible casting defects are described, illustrated and discussed, comment being made upon casting shrinkage and machinability. Improvements in annealing-oven construction and operation are reviewed and the records of 100 consecutive heats in different plants are tabulated. The materials for casting that compete with malleable iron are mentioned and its physical characteristics are considered in some detail.

Emphasis is placed upon the necessity for the elimination of casting shrinkage and a brief summary is made of the most important features of the paper. [Printed in the January, 1922, issue of THE JOURNAL.]

THE DISCUSSION

R. H. SHERRY:—Mr. Touceda should have laid more stress upon the matter of the design of malleable castings. Some very peculiar things appear in the old books on automobile design. For example, one will find the steering-knuckle represented as being machined at right angles, which would cause it to fracture after comparatively short service. Some of the things perpetrated in tools show the same faulty design that often is exhibited in castings. It should be one of the duties of the metallurgist to use his best judgment in examining the designs for malleable castings, before he even undertakes their consideration.

I have in mind one casting that was stated by a prominent automobile engineer to be impossible to make, but by a minor change, I think it was an increase in thickness of about ⅛ in., a considerable sum of money was saved by using that part. I recall also the clutch hub of a certain popular car which is a malleable casting with cyanided splines. A number of them are placed on a steel rod and rotated over the cyanide pot. The malleable iron cyanided very readily and with perfect satisfaction. I believe this form of hub can be made at much less cost than any other.

ENRIQUE TOUCEDA:—The design of malleable-iron castings with sections disproportionate in such a way as to make them almost impossible to be cast free from shrinkage is the one big difficulty that confronts the foundryman. To produce sound castings he is forced to resort to almost every expedient known to the molding art. A complicated casting of disproportionate sections should not be regarded in the light of a single casting having one or perhaps two gates, but as a mass of metal made up of a lot of separate castings united by thinner sections that may be considered as gates joining heavier

parts and actually a part of the casting itself. If two heavy parts are united by one that is too thin, as is the case with many automobile castings, the thin portion will freeze before the others have had their full quota of metal. If the uniting part is thickened slightly, this defect can be overcome.

C. S. SCHROEDER:—With reference to the approximate limiting thickness of a malleable casting, how thick can such a casting be made and still retain the high tensile-strength and elongation that are obtained with ½-in. sections?

MR. TOUCEDA:—In the case of sections having a thickness of 1 in., it is not believed that any consequential variations in strength will be found in any part of the section. In a section 2 in. thick, some difference in strength would be found between a specimen taken close to the surface and one from the center of the section. This might amount to as much as 4000 lb.; but, inasmuch as the average ultimate-strength of malleable cast-iron is 53,000 lb. per sq. in., the strength of the bar cut from the center would still be much higher than the requirements laid down by the American Society for Testing Materials. Being cut from metal all of which is very near to the neutral axis, the loss in strength would be negligible. There is no difficulty in the breaking-up of the hard iron carbides during the annealing, irrespective of the thickness of section, as this conversion is a function of time and temperature and will be complete in all cases, provided the composition is correct from a metallurgical viewpoint.

MR. SCHROEDER:—But is not the outer structure ruined by prolonged annealing?

MR. TOUCEDA:—Not at all. Many believe that the strength of malleable iron lies mainly in the skin, owing to the surface metal being decarbonized, but many tests on machined samples prove that this is not so. It is really a matter of soundness whether duplicate bars, one machined and the other not, will show a difference in strength. One reason for the old belief was that tests were made on bars that lacked soundness due to shrinkage. When the skin was removed by machining, the test was made on that part of the bar that lacked strength because of this defect. The great value of the malleable casting lies not only in its high strength and ductility but in the ease with which it can be machined. There is no other ferrous product that can be machined at such a high rate of speed. Many companies attain a rate of 150 ft. per min.

CHAIRMAN F. P. GILLIGAN:—Do automobile engineers show any lack of response to requests for alterations of design that will affect their products?

MR. TOUCEDA:—There has been an improvement within the last year. Engineers are beginning to realize more fully the reasonableness of such requests. We can certainly produce better results if designs are better.

CHAIRMAN GILLIGAN:—Has there been any trouble with the factory people in that respect?

MR. TOUCEDA:—The trouble is all along the line and not with the manufacturer particularly. In his design the engineer considers mainly what sections he must have to assure sufficient strength in service, in many cases without realization of or regard for the problems that may arise in connection with the production of the castings. I am glad to state that our requests for modifications in the design of patterns are now met in a much more tolerant manner than formerly. In one recent instance a pattern was changed at our request and success immediately followed.

PASSENGER-CAR BRAKES

BY J. EDWARD SCHIPPER

STATING that the problem of deceleration is just as important and necessary of solution as is the one of providing car-acceleration ability, the author gives a comprehensive survey of present braking practice and outlines future requirements and possibilities.

Design factors are considered at length, as well as the subject of what constitutes uniform and effective braking-power, various illustrations and descriptions being included of different types of brake. Brake-actuating means, the calculation of brake-drum size, car-stoppage ability, brake equalizers and brake-linings are commented upon in some detail.

The future of brakes is discussed with reference to the use of the engine as a brake, four-wheel and front-wheel brakes, the servo principle of brake operation and various novel braking methods. A brief summary of what is considered good practice with regard to truck brakes is appended. [Printed in the April, 1922, issue of THE JOURNAL.]

THE DISCUSSION

H. W. SLAUSON:—I was much interested in Mr. Schipper's reference to the effect on the transmission and clutch bearings of using the engine as a brake. It seems to me that this use of the engine is necessary, even though we do have sufficient heat-dissipating design in the brakes themselves, especially on steep grades. If the transmission is in second, in low or even in high gear and the skid starts, to let the clutch back in and start the wheels revolving gives a much greater factor of safety in descending a steep hill than if the gears were in neutral. If the transmission and clutch bearings are of sufficient capacity to drive a car up a steep hill without becoming overheated, why is it that they cannot take the reverse thrust?

One weakness in the design of some brakes is the tendency of the anchorage to break; that is, the point at which the brake-band is secured and prevented from revolving when the brake is applied.

WILLIAM R. MCCULLA:—In regard to using the engine as a brake, I have had considerable experience in contest work in endeavoring to get a large number of miles per gallon of gasoline. In one recent test a light car traveled 106 miles per gal. under official supervision. The previous day it traveled 114 miles per gal. I have been advised that this car can travel 160 miles per gal., but this is accomplished by stunt driving. Another make of car that was attempting to beat this record recently was equipped with an over-running clutch, which was most satisfactory. Most of the economy was obtained by changing the gears into neutral and coasting nearly ¾ mile after a certain speed had been attained. In driving in the mountains, if the engine is used as a brake, the gasoline consumption is increased very materially. Provision should be made for proper brakes instead of depending upon the engine. In one English car, when the engine was used as a brake, the carbureter admitted pure air instead of a mixture of gasoline and air. This was of great assistance in cooling when driving in mountainous regions and, of course, it did not increase the gasoline consumption or affect the lubrication by sucking raw gasoline through the combustion-chamber.

N. L. DOD:—The insurance interests are considering using the acceleration and deceleration values for liability insurance against collisions. The present high-priced values are due to the fact that the dollar-for-dollar value of materials in cars seems to be about an average.

The formulas that have been submitted are too complicated or have indeterminate values that involve a judgment factor, to be determined by the man doing the rating, which of course is not advisable. We have made experiments with four-wheel brakes and find that in going down-hill the greater proportion of the load of the car is transferred to the front-wheel brakes; so, if the front-wheel brake-drag with a certain weight of car is of the same capacity as the drag on the rear-wheel brakes, the breaking efficiency probably would be 50 per cent greater than with the same total braking capacity on the rear wheels alone.

PROPELLER-SHAFT BRAKES

CHAIRMAN H. W. ALDEN:—Mr. Schipper says that propeller-shaft brakes are coming in. I think there is no doubt about it, particularly on the smaller and lighter cars.

K. G. HOWELL:—My experience with propeller-shaft brakes covers some 30,000 miles. During that time I have never had a particle of skid caused by the application of the brake, which has a drum 12 in. in diameter and about 3 in. wide. I have had some trouble with brake-lining, but one company manufactures a lining that is entirely satisfactory. I have found that a transmission brake is most satisfactory on the average road. I usually drive at an average speed of 45 m.p.h. and my experience covers 6 years. The transmission brake can always be depended upon on a wet, slippery surface and on a turn. It has the easiest type of adjustment; a small wing-nut adjustment is necessary only once in about 2000 miles of travel.

R. W. A. BREWER:—It seems to me that an engine is for propelling a car and that a brake is for stopping it. It surely is within the province of the engineer to design some mechanism that will operate satisfactorily for the purpose for which it is designed. In this country we are more familiar with the type of brake that is attached to the rear wheels, but there is some interest now in the direction of a proper propeller-shaft brake. I endorse what Mr. Howell has said about the propeller-shaft brake. From many viewpoints it opens possibilities for good engineering design that cannot be attained in the ordinary type of rear-wheel brake. To mention a few of these points, one is the variable amount of friction that cannot be avoided with the rear-wheel type of brake. This friction varies with weather conditions. For example, one cannot depend upon getting the brake into effect when the brake-bands become wet. Furthermore, in many designs of the rear-wheel type of brake, the adjustment and consequently the braking effort vary with the number of people in the rear end of the car. That makes it always a matter of more or less uncertainty how the brakes will operate in an emergency. With a propeller-shaft brake those conditions do not obtain; one always has the same coefficient of friction on the brake-drum. In many designs one can adjust the brake-drum from the driving-seat, which is an advantage in mountainous country.

In hilly countries the problem is entirely different than in the major portion of American motoring, but we must design cars for any sort of condition that reasonably may be expected. One cannot say that because 90 per cent of the cars run on smooth, level roads the other 10 per cent can be neglected. During four years of driving in California I used four different cars and believe that not one of them delivered what should reasonably be expected from a reliable braking system. The brakes could not be depended upon to operate through a day's run without cooling them in a stream or using the engine as a brake. Acting as referee for the American Automobile Association Contest Board in California on reliability runs, I have penalized the drivers in many cases for doing the very things that I mention. The standard cars would not negotiate the grades met in an ordinary day's run without the necessity for stopping and doing something to the brakes.

SANFORD EVANS:—The brake is primarily a means of dissipating, as heat, the energy that is stored up in a car when it has gained a certain momentum, but many braking systems seem to indicate that we have forgotten this. Many of the present brakes will stop a car readily enough and many engineers test cars on the basis of attaining a certain speed, applying the brakes and seeing how quickly they will stop the car. It appears to me that the reason we use the engine as a brake is that ordinary brakes will not withstand the constant drag that obtains in going down a long hill. The energy stored in the car requires sufficient heat-radiating surface. Almost any brake-lining will overheat. In my opinion, it injures an engine to run it at high speed when driving down-hill with the ignition either shut off or on. We would not open the throttle when the car is standing still and let the engine turn over at 1200 or 1500 r.p.m. under absolutely no load. I believe that braking with the engine causes even more harm than idling the engine at a high speed. If the transmission brake, which is probably the only "equalizer" that approaches perfect equalization, can be designed to radiate the heat that must be dissipated through the brake, it is a promising medium of experiment.

H. M. CRANE:—Mr. Schipper reproduced in Fig. 25 of his paper a brake design that we recently developed, and I wish to explain the principles upon which this brake design was evolved.

For a smooth-acting friction-device of any kind, the thing of greatest importance is low unit-pressure. That is attained in a variety of ways, such as having large areas or having the areas traversed at high speed. That is an advantage obtained with the transmission brake that operates on American cars, at from 3½ to 5 times the speed of a rear-wheel brake. It is attained also by being certain that the apparent pressure is uniformly distributed over all the braking surface. That is one of the most important considerations and it is where the shoe type of brake invariably fails. It is a place also where many of the band brakes fail, due to a design of anchorage, that causes a wedging at the anchorage, or to a design that creates excessive pressures at the point where the bands take hold.

On the service brake we have only 4 to 5 in. of pedal motion with which to work. If we use more pedal travel, the car becomes uncomfortable to drive. We also have a limiting pressure. We are limited to a pressure that can be applied by a driver comfortably. Many drivers are women who are neither heavy in weight nor powerful in muscle. We must see that this 4 to 5 in. of pedal motion is transferred in the form of motion and multiplied to the necessary amount at the friction surfaces without loss. If there is springing of the parts or lost motion, in just that degree we are reducing the possible multiplication that allows the use of a low pedal-pressure.

On the transmission brake shown in Fig. 25 of Mr.

SIDE GUIDE AND GRIPPER

Schipper's paper, the actual figures on a 5500-lb. car weighing 6000 lb. with a normal number of passengers show that it will slide the rear wheels of the car with less than 50-lb. pressure on the brake-pedal. Part of this result is due to the fact that the entire multiplication is obtained at the point of application of the power, so that there is practically no strain in the controlling parts; part is because the rear-axle ratio is 4 to 1, which gives a very high peripheral speed on the drum; and part is for the reason that the drums in question on this transmission shaft are as large in diameter and area as the rear-wheel drums on many American cars of 5 or 6 years ago. They are not so large in diameter as some rear-wheel brakes on the present cars, but they are as large as one can locate safely under the floor of a car, and that is where the transmission brake must go.

As a designer of heavy cars, I say that the idea that a brake can be or should be designed for maximum possible conditions in mountainous countries without the assistance of the engine is an impossible consideration. With a car weighing 3 tons in its running condition and figuring the amount of heat that must be dissipated, the result shows immediately that no size of drum that can be placed readily in the car, even with the assistance of water-cooling, can be expected to take care of the braking requirements. On the other hand, in the well-designed engine we have everything present that is needed to make a good brake. The resistance of an engine turning at high speed with the throttle closed, as it should be in braking, is very largely due to the pumping action of the cylinders. In other words, we have a reversed airbrake. We have an efficient oiling system taking care of all the moving parts. We have a radiator that might as well be used and that would otherwise be doing nothing. There is no question that this can be done with impunity in a well-designed engine. I have records of cars used in Alpine work where no difficulties of any kind ensued when using the engine as a brake; there were no oiling troubles, no excessive wear and no troubles of any other kind. There is probably enough room on very small cars to put on brakes that will do the work.

To revert to the small amount of pedal motion available, this increases very greatly the efficiency of the transmission brake as compared to rear-wheel brakes because one deals with a single set of operating parts and not two sets. The multiplication of the number of operating parts means more lost motion, and makes it impossible to use the same degree of multiplication that one would use on a single set of parts. To go further, I call attention to the serious effect that this has on the attempt to use four-wheel brakes. With four-wheel brakes it is almost impossible to get the adjustments such that the braking system is really efficient, unless this is done by experts. I think that the four-wheel brake is not desirable in any sense for the average man and that, in the main, it will result in more accidents than it will ever prevent.

L. F. HOWARD:—Recently, we have developed an air-brake for automobiles. Will Mr. Schipper give his reasons for saying that "the hydraulic system probably shows the greatest promise at present because of its simplicity and also from the standpoint of perfect equalization"?

FOUR-WHEEL BRAKES

F. S. DUESENBERG:—I do not agree with Mr. Crane that four-wheel brakes might be the cause of more accidents. I feel that a four-wheel brake is likely to avoid accidents, provided it is kept reasonably well adjusted and equalized.

I made a trip about 2 years ago that included many hills from 3 to 4 miles long, with a standard car having two rear-wheel brakes. The emergency brake had an inner and an outer band. In going down hills I would use one brake until the drum was red-hot; then I would apply the other brake and burn that lining. When I approached the bottom of the hill I had no brakes and, consequently, I used the engine as a brake. The car was not in the best of condition, but the trouble was that a large quantity of oil was pumped. I was pretty thoroughly disgusted with the two-wheel brake. When we decided to adopt the four-wheel brake, the first trial was made on those same hills. In going down we found that we could travel at any desired speed and when we reached the foot of the hill the brakes were no hotter than would allow one to hold one's hand on the drums.

We are using drums that are drop-forged and rather expensive to make. We rib them very heavily. In that way we get a cooling capacity that we consider is 10 to 12 times greater than one can possibly get on a two-wheel brake having an inner and an outer band; with the latter type one cannot get rid of the heat. We are using a hand-lever with the propeller-shaft brake, but have experienced some trouble. We did not realize at first how great the pressures and service are that are put on the bearings and the other parts that are connected to the propeller-shaft. However, after the test we have made recently, we feel satisfied that this is only a point of minor importance and easy to overcome.

We feel that we can stop a car with four-wheel brakes in practically one-third the distance required with a two-wheel brake. That is true particularly on slippery pavement. Many say that the four-wheel brake should give different service front and rear. We did not consider that of any importance at first but after experimenting decided to put a stiffer spring on the front brakes so that the rear brakes would be applied first. Since we have been doing that there has been absolutely no objection to the braking.

We have tried the transmission brake on some long hills. Unless one uses metal lining, which I think is objectionable in most cases on account of the noise, there is a considerable tendency toward burning if one uses no other brakes. We never use the hand-brake except for locking the car. Many things can be said in favor of the hydraulic brake, but I believe that a mechanical brake, properly applied and possible of being equalized easily, should be about as satisfactory as a hydraulic brake, except that the hydraulic brake eliminates many moving parts and lessens the chance of unequal braking.

BRAKE AREA FORMULA

E. FAVARY:—The practice of manufacturers of announcing in their literature that their cars are equipped with a certain brake area, gives no indication of the efficiency of the braking system. Unless it is known how far this brake area is located from the center of the wheel, it is impossible to find the effort required, or the necessary pressure between the brake-band and the brake-lining to lock the wheels. For this reason I think that the formula for finding the diameter of the brake-drum, given in Mr. Schipper's paper, is of little value to the designer. I present two examples to show how misleading any formula is that enables us to find only the brake area, assuming in one case a brake width of 1¾ in. and in the other case a width of 2½ in.

With a brake width of 1¾ in. and a maximum car-weight of 4000 lb., we have according to Mr. Schipper's formula.

$\{[(4000 \times 0.045) \div (2 \times 1.750)] + 1.500\} \div \pi = 16.800$ in.

This is the diameter of the drum. If the brake width is 2.5 in., the drum diameter is 11.9 in. The surface area of the brake is approximately 90 sq. in. in each case, but the brake-drum diameters differ greatly. Assuming the wheel diameter to be 34 in., the radius, R, then being 17 in., and the load on each rear wheel to be 1250 lb., when the wheels are locked there is a pull at the surface of the tires of 1250 x 0.6 = 750 lb., where 0.6 is the coefficient of friction between the tire and the road surface. The torque created by the wheel is $750R = 12{,}750$ lb-in. To find the force required at the periphery of the brake-drum to lock the wheels, the torque must be divided by the radius of the brake-drum, which is 8.4 in.; hence, this force is 1514 lb. If the coefficient of friction between the brake-lining and the brake-drum is 0.25, the normal pressure between the brake-drum and the brake-lining in $1514 \div 0.25 = 6056$ lb., and the actual pressure is $6056 \div 90$, or 67 lb. per sq. in. approximately.

In the second case, where the brake-drum diameter is 11.9 in., we find a pressure of 95 lb. per sq. in. between the brake-lining and the brake-drum by carrying the calculations through in the same manner. This is almost 50 per cent more than in the first case, while the radiating surface, that of the flange attached to the wheel, is smaller by $[(0.7854 \times 16.8^2) - (0.7854 \times 11.9^2)] = 110$ sq. in. Hence, the lining will wear-out sooner, the drum will become hot more readily and, if the operating linkage is the same, the foot-pedal pressure would need to be 50 per cent greater to lock the wheels. I think that a much better indication of brake efficiency is the pressure per square inch between the brake-lining and the brake-drum that is required to lock the wheels when the car is fully loaded and traveling on a level road.

A. M. WOLF:—In the Saurer construction the engine has a sliding camshaft that alters the normal operating cycle of the engine and makes it more efficient for braking purposes.

At the end of Mr. Schipper's paper reference is made to motor-truck brakes. In this connection I firmly believe that, where a large amount of energy is to be absorbed in the brake, the Mack and the White construction of making the brake a separate unit is very commendable. In this way the brake can be supported adequately at each side by a bearing and great rigidity secured, which is not possible in the ordinary overhanging propeller-shaft brake.

On some cars using a torque-tube and a transmission brake the universal-joint is relieved of braking stresses by placing the brake to the rear of the joint, supporting it by the front end of the torque-tube instead of by the transmission.

We find two forms of construction in the mounting of the brake-drum on the rear-axle hub. I believe that we should mount the drum adjacent to the integral flange on the hub that, necessarily, must be toward the inside in this construction. Where the flange is on the outside, the drum is bolted against the wooden hub of the wheel. This cannot be relied upon for such accuracy as when it is mounted adjacent to the integral flange.

The ordinary stamped brake-drum is, in many instances, lacking in concentricity, which of course prevents proper brake operation. Sometimes the braking surfaces of the drum are machined, which insures proper operation. Where the expense is permissible, there is no doubt that the best construction is obtained by the use of the forged drum with radiating ribs on the exterior, which stiffens the drum at the same time; also, the braking surface can be machined or ground.

Mr. Schipper's paper makes no mention of the multiple-disc type of brake, such as was introduced by Lanchester in England and by Metz and Parker in this Country. Sufficient surface is obtained easily by this method, and I believe it has great possibilities. Located at the back of the transmission, where there is plenty of room, and where it also becomes unsprung weight, this construction should make an ideal service-brake.

Care should be used in the lubrication of the brake rigging. The practice of locating grease-cups where they are absolutely inaccessible should be replaced by other construction such as the use of self-lubricating bearings.

Two other methods of energy absorption have been used. These are the Saurer fan brake that dissipates energy in a similar manner to that of a fan dynamometer, and the hydraulic method as exemplified by a construction in which the constant-mesh gears of the transmission are encased so as to form a gear pump, braking being secured by a valve retarding the flow of oil between the inlet and outlet of the pump.

J. E. SCHIPPER:—Mr. Dod spoke of the greater efficiency of the front-wheel brake when four-wheel brakes are used. The efficiency of the front-wheel brake is not determined exactly by the division of weight between the front and rear axles. The front-wheel brake has the effect of the forward rotating movement in addition to the weight of the car. This tends to throw the car over on its forward end when the front-wheel brake is applied, and that is a very considerable factor. In a number of the letters received on four-wheel braking I notice that the engineers believe that the front-wheel brakes should be made a little less efficient than the rear-wheel brakes to facilitate steering. It is noticeable that a man who is accustomed to rear-wheel brakes alone will have trouble in steering if he tries to apply his brakes at the time he is rounding a corner, as he would do when operating rear-wheel brakes. This will tend to throw the car into a kind of front-wheel skid. After practice he will apply his four-wheel brakes before he reaches a curve.

With reference to Mr. Evans' remarks on using the engine as a brake, how many of us would like to drive our cars in second gear at the rate of 20 to 25 m.p.h. for any extended length of time without something to hold the transmission from jumping out of the chassis?

In answer to Mr. Howard's question on hydraulic brakes as compared with airbrakes, I might amend my paper to read, "a fluid system," instead of "hydraulic," because the principle that I wanted to bring out applies equally to air, water or oil. I wished to emphasize that in a fluid system the pressure is balanced at all points by the very nature of its fluidity, this providing equalization.

DROP-FORGING PRACTICE

BY J. H. NELSON

THE author discusses drop-forging practice from the standpoint of the materials used, and strongly advocates a more rigid inspection and testing of raw products to determine their fitness for use in making automatic forgings.

Seven specific possibilities of actual difference between drop-forgings that are apparently identical are stated, the requirements of the inspection of raw stock are commented upon, and the heat-treatment and testing of finished forgings are considered at some length.

Tabular data of the chemical analyses and physical properties of 107 different heats of carbon-steel used recently are presented and show a variation in drawing temperatures of 140 deg. fahr. in steels of practically the same chemical composition to meet the same physical-property specification, based on more than 1000 tests on this grade of steel taken from production stock. The concluding summary has five specific divisions.—[Printed in the March, 1922, issue of THE JOURNAL.]

THE DISCUSSION

J. H. NELSON:—We have found from our experience in producing quality drop-forgings that it is not safe to say that all steel conforming to a certain chemical specification will respond in exactly the same way in all heats of that particular grade. In fact, we have found from our experience that the result is contrary to that. That a steel is classed as a 0.40 to 0.50-per cent carbon-steel does not mean that a melt of this particular steel will respond in a like manner to one from the next shipment received from the same or another mill. To minimize our losses in the shop and give our customers the quality they desire, we have found it absolutely necessary to inspect very closely and to keep segregated throughout the yard and shop, every melt of steel that is received from the mill. Within the last year, I have been keeping accurate records of our operations to determine exactly whether our losses are due to improper heat-treatment or whether they are chargeable to the steel. I have compiled the information given in this paper from these records, thinking that it might be of some assistance to others who find their heat-treatment losses running rather high.

The two causes of heat-treatment losses are imperfect or mixed stock and imperfect equipment. We are satisfied that, while our heat-treating equipment is not perfect, it is far above the average and tests made on production work have proved that with a uniform heat of steel we can turn out a uniform product. We have eliminated the personal equation by placing our furnaces under automatic control so that the over-running of heat is eliminated. Therefore, to study these losses, we have kept accurate records of the Brinell-hardness of every piece treated, as well as of the physical tests on each furnace-charge. From these I have compiled the results given in Table 1, which includes 107 different melts of steels received from a number of steel mills. I have grouped these according to the draw heat necessary to meet a definite specification. We have found, as shown, that in order to minimize our losses it is necessary to draw from 980 to 1120 deg. fahr. and to keep within the Brinell-hardness limit specified, namely, 228 to 248. To emphasize that more forcefully, Table 2 summarizes the results of Table 1, giving the average of both the chemical and physical properties of the different groups and showing the number of tests made.

Better physical properties could be produced on these steels if we were allowed a higher Brinell hardness. However, it is to be noted that the average, representing nearly 1100 tests, gives a variation between the maximum and the minimum elastic-limits of about 5000 lb. per sq. in., and in tensile-strength a difference of about 6000 lb. per sq. in. This is conclusive proof that the draw heats used were necessary to bring these steels to the common basis. It is also to be noted that the Brinell hardness of these steels is very close.

This demonstrates clearly the dangers of attempting to draw conclusions from a single melt of steel. If some one had been investigating 0.40 to 0.50-per cent carbon-steels to produce certain results and had used a melt of steel from Group *A*, his conclusion would have been that 980 deg. fahr. is the necessary drawing temperature on all 0.40 to 0.50-per cent carbon-steels to meet this definite specification. If another investigator had been using Group *H*, he would have arrived at an entirely different conclusion with respect to draw heats; so, in discussing the relative values of steels, to my mind it is very necessary that a large number of different heats be investigated, in order to determine the average condition that can be obtained. Nothing of conclusive value can be produced from a few tests on a single heat or melt.

R. H. SHERRY:—In what type of furnace were these draw heats made?

MR. NELSON:—In an under-fired furnace.

MR. SHERRY:—In drawing hardened pieces of the nature of crankshafts we have got away from some of this variation in Brinell hardness and in the physical properties of the same type of steel by the use of the lead pot on a large scale, one from 6 to 7 ft. long and 20 to 24 in. wide. Of course, we do have the variation that Mr. Nelson mentions, but I wonder if there is not some further chance for research along this line that will give a direct comparison. These data are very valuable from the furnace standpoint, but so far as I have been able to ascertain, it is very difficult to obtain a uniform draw in the several varieties of furnaces that seem to be uniform in control. By the use of the lead pot with its exact temperatures, in the case of heavy pieces using two pots and carrying from one to the other, we have been able to hold that range considerably closer than I notice has been done in this particular type of furnace. Has Mr. Nelson had experience along that line and is that not a factor in the variation of the figures?

MR. NELSON:—I am a firm believer in the physical properties, including Brinell hardness and functions of temperature, rather than in the means of obtaining that temperature. To my mind it is immaterial whether one uses a lead pot, or an electric, gas, oil or other type of furnace, provided one produces the temperature with which one is working. These furnaces are balanced very carefully before they are turned over for production work. They are balanced by using pyrometers that have been checked accurately. A pyrometer is placed in each corner of the furnace, which is the part most difficult to heat, and also in the front and in the rear center of the furnace. These six pyrometers must agree within 5 deg. before the furnace is turned over to production work.

In our control we have furnaces that are run by two pyrometers; one in the front center and one in the rear center. We can throw our control instrument on either fire-end. When a charge is pulled and a new charge is put into the furnace, the front of the furnace cools down more rapidly than the rear and the control instrument is thrown on the rear end when the furnace starts up. A two-point recording instrument shows a slight variation between the front and the rear until the desired temperature is reached, after which the two fire-ends run a straight line, which demonstrates clearly that we are actually holding to the temperatures indicated.

MR. SHERRY:—I realize that Mr. Nelson has perfect control in those furnaces because I have heard much about them but, regardless of pyrometer control, we had an experience with a certain type of furnace that was absolutely uniform. I ran it with six or seven thermocouples under different portions of the furnace and yet obtained variations. There are other factors beside pyrometric control. For instance, with two large sections piled one on top of the other, which sometimes occurs, it

takes longer to heat through from steel to steel than it does to heat with a direct application of the heat to the surface of the steel. I mention this as being possible. I found variations in the very best types of furnace, those having the most uniformity.

W. F. GRAHAM:—Without questioning the accuracy of the control that Mr. Nelson mentions, which is undoubtedly very good, I support Mr. Sherry in his idea that lead-pot furnaces do give a remarkable accuracy compared with any other type in which the heat is not conveyed to the metal by molten metal. My experience on fairly large forgings running in the neighborhood of 0.35 or 0.45 per cent carbon has been that the most accurate results are obtained when those forgings are quenched from a lead pot and drawn in a lead pot. The more intimate contact of the heating medium with the work, particularly when it is put through in a commercial way and there is more than one piece in the pot, gives a more uniform hardness.

Are not these variations that Mr. Nelson mentions, variations in hardness that will occur unless one varies the drawing temperature, a function of the carbon and the manganese-content of the material? With both the carbon and the manganese on the low side of the specification, one cannot expect to get the same result as when one has them both on the high side. This comes back to the old argument that is advanced for a five-point range in carbon, which we know is practically impossible to obtain on a commercial scale from the steel companies; in fact, the heats will not run as uniformly as that in many cases. My experience is that it is absolutely necessary to take into account the percentage of carbon and manganese, and to vary the drawing temperatures accordingly over a range of about 200 deg. in medium-carbon steels. In this connection I have found that on hardened and drawn work covering the range of S.A.E. No. 1035 steel, at about 0.30 per cent carbon there is a very sharp drop in the hardening power. If the steel runs a little below that in carbon, with the manganese down fairly well, one cannot expect to get good results. One practically would need to eliminate the draw to obtain the average specification of hardness on which one is working.

With the S.A.E. Standard range of 10 points of carbon and the manganese from 0.50 to 0.80 per cent, although Mr. Nelson says he uses a manganese range of 0.60 to 0.80 per cent, I have found that the drawing temperatures necessarily will be varied in proportion to the manganese and carbon-contents for all practical purposes, unless the work is put through in individual heats, which is the desirable condition. On small lots I have made it a practice to divide the work into three classes; to take a 0.30 to 0.40-per cent carbon-steel and group the heat into three classes, from 0.30 to 0.33, from 0.33 to 0.37 and from 0.37 to 0.40 per cent, taking into account the manganese-content. How does that correspond with Mr. Nelson's idea of it?

MR. NELSON:—The chemical composition of every individual heat is given in Table 1 of my paper. In Table 2, which is the average of the chemical composition in the various groups as well as of the physical properties, we find the variation in carbon-content from Groups *A* to *H* to be 0.43 to 0.47 per cent. The averages being so close must mean that the carbons in the particular group are very nearly the same or that there is a certain proportion in each group that is high and a certain proportion that is low, to give us the same average; consequently, it is sufficient to examine Table 2. Hence, we find a variation in carbon from 0.43 to 0.47 per cent.

In manganese, we find a greater variation. Group *A* is 0.65 and Group *H* is 0.77 per cent but, if we eliminate Groups *A* and *H* and compare from Groups *B* to *G*, we find from 0.69 to 0.75 per cent is the maximum variation. The fact that a steel is analyzed and found to contain about 0.40 per cent carbon and another contains 0.50 per cent carbon does not mean at all that the one which contains 0.50 per cent carbon will require a higher draw than the 0.40-per cent carbon-steel.

MR. GRAHAM:—In Table 2, have Groups *A* and *B* as compared with Groups *G* and *H* received the same quench and the same draw?

MR. NELSON:—Groups *A* and *B* have received the same quench but Group *A* is drawn at 980 deg. fahr.; Group *B* is drawn at 1000 deg. fahr. to meet the required specifications. We use the same quench on all these steels.

MR. GRAHAM:—Is that a standard treatment or is it varied with each individual heat?

MR. NELSON:—We investigate every individual heat and determine what heat-treatment it must receive.

MR. GRAHAM:—Then this is really an average of the heat-treatments as well as an average of the other results?

MR. NELSON:—Group *A*, as given in Table 2, is an average of six different heats of steel in Table 1. All these different melts of steel in Table 1, Group *A*, received a 980 deg. fahr. draw to comply with the specifications, and so on throughout. It was absolutely necessary to use that heat because, if drawn at a higher or a lower heat, one would lose a certain percentage of the product. This is the best average heat we could get to maintain a minimum loss.

With regard to Mr. Sherry's remarks, we do not pile materials in our furnaces. Materials are put into the furnace in an orderly manner so that we can get a uniform heat. I agree with him that, if materials are piled in a furnace, uniform heating is difficult to attain. Our furnace charge is not a hit-or-miss proposition; it is composed of crankshafts of the same kind. One row is placed across the hearth of the furnace in the rear and another across the hearth of the furnace in front; there is no piling.

MR. SHERRY:—That particular case I mentioned was rather a form of illustration. I believe that if one should look into the average drawing furnace, where the product is at a visible red heat such as I have seen on crankshafts calling for higher temperatures on the draw heat, even with the best regulation one would see some variation in the color indicating a variation in temperature, merely from a slight change in the position of the shafts. It does happen. On the Liberty-engine crankshaft, drawn in a very accurately built furnace giving a high degree of heat uniformity and held under thorough control, it was found necessary to run many through a second time.

H. J. STAGG:—Mr. Nelson has told of variations that occur, but it seems that these variations do not occur in his shop. The conclusions that the steel manufacturer must reach is that the variations are due to manufacture. Has Mr. Nelson any thoughts on the why and wherefore of these difficulties he has pointed out?

MR. NELSON:—I look upon steel manufacturing as being very much on the same order as bread baking; that is, the chemistry of a loaf of bread may be perfect but in the baking of that loaf of bread, if the heat is raised a little too fast, is too high or not high enough, one would not get good bread. In the same way I believe that the matter of heating and other things in furnace control that influence the making of a heat of steel have a decided effect on the physical properties of the steel pro-

duced. We will occasionally get a heat of 0.40 to 0.50-per cent carbon-steel that will give exceptionally high values. We will then get another heat of steel that we cannot use; both will analyze perfectly, chemically. Again, we will get a heat of a 0.30 to 0.40-per cent carbon-steel that will give us better physical properties than can be produced with the average 0.40 to 0.50-per cent carbon-steel. But it has so happened, I believe, that there was a balanced condition in the furnace. The steel manufacturer probably knows just what that balanced condition is.

MR. STAGG:—Has Mr. Nelson been able to detect any difference in the raw materials coming from the mill; that is, any physical or microscopic difference in the material with which he started that would enable him to predict what result he would get, what the material would do? One must be able to tell what that difference is; things are not so intangible that we cannot determine them.

MR. NELSON:—I agree with Mr. Stagg, although I am not ready to state what the difficulty is. However, I am keeping very accurate records as to the practice of the different mills in addition to these other records. We find that Mill *A* will invariably require a low heat, Mill *B* will require a high heat and Mill *C* will strike a middle point; that is, for the same identical chemical composition, there is a difference in the practice in the different mills such that one can trace them in that way.

C. N. DAWE:—With regard to this variation that Mr. Nelson mentions, I note that his Brinell hardness is held to about 10 points in all these tests; in some classes of our work we allow a range of 50 points in Brinell hardness. If we decide on a certain draw heat for a particular class of steel, the variation in the Brinell hardness may be as stated; but, if we tied it down to 10 points, we would need vary our draw heats too much.

In answer to Mr. Stagg, I would say that I have seen a heat of crankshaft steel that had the proper chemical composition and, so far as we could find out, was metallurgically correct, but we could not harden it. A representative of the Carnegie Steel Co. tried two or three times to harden these crankshafts; he even used special care to pick out the crankshafts that fell midway between the limits, but we could not harden them. The company took some of the crankshafts back to the mill, and stated that it could not answer the question. The peculiar thing about it is that, after the crankshafts lay around the yard for a number of months, we treated them and they came through all right. Recently, we have heard of the seasoning of forgings. There was a claim current that by allowing gear forgings to season for a certain length of time, much of the warpage would be eliminated. It is logical to assume such a thing. I can vouch pretty well for the furnaces that we ran the crankshafts in later; they are very carefully watched and checked. Those are the facts of the case. The point I was trying to bring out is that there was apparently no method of accounting for the action of this particular heat. I have heard of taking heats that have been allowed to stand in the open hearth longer than they ought to; but one cannot get the results out of those particular heats, although one perhaps may never detect it in the analysis. I am not sufficiently informed in that direction; there may have been something of that nature which caused it.

MR. SHERRY:—Did Mr. Dawe use oil or water in quenching crankshafts? I presume Mr. Nelson used water.

MR. DAWE:—We used temperatures on 0.40-per cent carbon-steel from 1525 deg. fahr. up, and went as high as 1700 deg. fahr. to harden this particular heat without success. Water was used as a quenching medium in this particular instance.

MR. SHERRY:—We experienced the same thing in that stock and found later that we could put it through, but I believe Mr. Nelson is referring to current practice day in and day out.

THOMAS H. WICKENDEN:—I had a similar experience that bears Mr. Nelson out. During the war we made a large quantity of road track links for tanks. The steel used was of the same analysis that we are dealing with here, the physical properties required were similar and, if I recall correctly, the Brinell-hardness limits were the same. The lower Brinell-hardness limit placed on it by the Government inspector was 228; the upper limit of 248 was placed on it by the shop to eliminate trouble in machining. Rockwell furnaces of the latest design were used in which the temperature throughout was found to be very even. The heats were controlled by pyrometers and we feel reasonably sure that our temperature readings were accurate. The men handling these furnaces are doing it every day and become expert at it. We kept a record of the number of links that were inspected for Brinell hardness that would pass within the specification the first time and, after a little experience, 98 and 99 per cent of these links would pass inspection with one drawing heat. We found that low carbon and low manganese will explain much of the difference required in drawing temperatures, but we also found heats with exactly the same analysis that would require a drawing temperature perhaps 100-deg. fahr. higher than with another heat of steel to secure the same properties. We found also that, by using the same temperatures on batches from the same heat, we duplicated our results without much difficulty.

MR. NELSON:—I wish to confirm what Mr. Dawe said with regard to heats that are wild. We find those occasionally, not only in the carbon-steel, but in chrome-nickel steel. In our inspection work we have adopted the practice of using a 2½-in. coupon; it is Brinelled on the outside and, when the coupon is split to cut the test-specimen, it is also Brinelled on the center. Thus, a comparison between the outside and the center Brinell-hardness numbers will account for much of the variation. In other words, there is great variation in the hardening capacity of the same steels. A 0.40 to 0.50-per cent carbon-steel may harden through perfectly; another 0.40 to 0.50-per cent carbon-steel may have very imperfect hardening capacity, and that is true also of chrome-nickel steels. Usually, a chrome-nickel steel or any alloy-steel 2½ in. in diameter will harden through fairly well. Occasionally, we get heats that are simply impossible to harden except to perhaps ¼ in. depth; inside of that ¼ in. the steel is perfectly soft.

CHAIRMAN F. P. GILLIGAN:—The foregoing discussion shows that it is desirable to make preliminary tests before going into heavy production based upon such physical properties as may be published for the purpose of guidance only.

SPECTROSCOPIC INVESTIGATION OF INTERNAL COMBUSTION

BY THOMAS MIDGLEY, JR., AND W. K. GILKEY

THE paper is designed to familiarize automotive engineers with the general subject of spectroscopy, pointing out the various methods that can be employed to determine the actual instantaneous pressures obtained in normal combustion, the temperature-time card of the internal-combustion engine and the progress of the chemical reactions involved in normal and abnormal combustion.

The subject of spectroscopy is outlined and explained, illustrations being presented of different types of spectra, and spectroscopes and their principles are discussed.

The remainder of the paper is devoted to an outline of what the spectroscope can reveal about the nature of combustion. [Printed in the March, 1922, issue of THE JOURNAL.]

THE DISCUSSION

THOMAS MIDGLEY, JR.:—In our mental picture we always have imagined that the high pressure area of the wave-front of detonation was a hot luminous region; that, necessarily, high pressures were in regions that were hot, consequently luminous, and therefore that they could be studied with a spectroscope. The spectroscope is of little use in studying cold and non-luminous gases. Recently we have done some work which seems to prove that the high-pressure portion of the wave-front of detonation is actually cold, and among the unburned gases; that, as the gases burn, the pressure drops. This conclusion has been reached mathematically. The spectroscope will give negative evidence of high pressure under these conditions.

Some optical indications of high pressure have been obtained in our attempts to measure the detonation pressures. The spectroscope was focused to give the two sodium lines of normal combustion as indicated in Fig. 5 of our paper. Photographs were taken under normal combustion without any knock. The plate when developed was found to contain two very black lines. The plate then was remounted in precisely the same position it had when the photograph was taken and a micrometer screw was used so that these two lines might be shifted a little to the right or to the left. The engine then was set to knocking. The sodium lines immediately broadened on both sides of the two black lines. The broadening of a line indicates increase of partial pressure. The shifting of the line bodily indicates an increase of total pressure. These are facts. But we did not see that the line shifted to one side or the other; it simply broadened. The source of sodium in the internal-combustion engine is simply the sodium in the air. We could not use additional sodium.

This result was rather disconcerting; we expected the lines to shift toward the red. By means of the micrometer screw the plate was moved toward the right and toward the left. We found that, while the lines apparently widened on both sides, there seemed to be a sharp position toward the red side where the light changed from an intense yellow to just a dull splash, while toward the violet position this did not appear to exist. Assuming this to be the shifted position, calculations show that the pressure in the flame-front of detonation was either 2960 lb. per sq. in., plus or minus 200 lb.; or 4660 lb. per sq. in., plus or minus 200 lb. The discrepancy between 2960 and 4660 lb. per sq. in. is accounted for on two assumptions: either that the line is narrow or that the line is as broad as the original. This would make the difference. We felt that a photograph would be proof. To get the photograph it was necessary to use a stroboscope in connection with the spectroscope, and to show a portion of the line in synchronism with the crank angle.

A photograph of a single explosion is very difficult to secure. The width of the slit through which light is admitted must be reduced to 0.001 in.; the available light, therefore, comes through a slit 0.500 in. long and 0.001 in. wide. The most sensitive film we could get required 2000 such explosions to give results. A spectroscopic photograph of a single explosion seems within the realm of possibility, but we have not been able to get it yet. Meanwhile, we shall endeavor to show that the high pressure, as indicated by the spectroscope, will be less than that indicated by other means. This will prove that the high-pressure portion is in the cold part of the charge.

Spectroscopic investigation by men properly qualified will yield an enormous amount of data on internal combustion. This paper is presented rather to indicate what things might be found out with proper instruments and methods than to present any new and startling facts.

C. S. RICKER:—Were panchromatic or the ordinary type of photographic films used?

MR. MIDGLEY:—The films were panchromatic when working in the sodium region, because ordinary films are not sensitive to yellow light. Not only was the panchromatic film used alone, but the film was sensitized.

C. H. SHARP:—I question whether the authors have made their point as to the great utility of the spectroscope in the study of these problems. Admittedly, the pressure effect as shown by the spectroscope is very indefinite. As given in the paper, the effect of 12 atmospheres of pressure is to shift a line about 0.1 of an Angstrom unit. That is a very small amount and very careful comparison would be required to determine it. The wave-length of sodium light being about 5900 Angstrom units, a shift of 0.1 unit is about 1 part in 59,000. A very powerful and well-constructed spectroscope would be required to show it at all, and evidently nothing very definite along those lines has been obtained.

We should note that the law on which the measurement of temperature as made by the authors is based is itself based on the assumption that the radiating body is a black body or a complete radiator. When a deduction is made as to the temperature of an alcohol-air explosion-flame on the basis of its being a black body, it seems to me that we are straining things considerably. An alcohol flame, which may be different under high compression, does not approach being a black body in the air; in fact, it is scarcely luminous at all. With a flame containing an excess of carbon like an ordinary oil or gasoline flame, we have conditions which approach black-body radiation. This displacement law may be applied in this way but, for alcohol, certainly, the results ascertained would hardly prove anything one way or another. In that case the radiation would be largely the radiation of water vapor, I imagine. We know that water vapor is

a fairly good radiator in the infra-red region, and very likely it is the superheated steam that is doing a large amount of this radiation.

Why not use the other law of temperature and light, according to which the whiteness of the light emitted increases with an increase of temperature? By putting a stroboscopic disc in front of the window and comparing the color of the light as given out by the explosion with the color of the light of an incandescent lamp having a rheostat in series with it until a color match is obtained, why is it not possible to get a measure of the average temperature of the exploding gases at the instant the window is uncovered by the stroboscopic disc? In other lines of work, we know that a very close comparison can be made between two temperature measurements in this way when the radiating body is a black body or a gray body that is not a selective radiator. Note that I suggest a "color" match and not a "brightness" match as in ordinary pyrometry. I think that the difficulties of carrying out this method would be far less than those of carrying out the determination of the position of the wave-length of maximum radiation, which, after all, is not a very definite thing. One must get the top of the curve, and it is more or less flat-topped, especially in the infra-red; it is not extremely definite, but the color match is a reasonably definite thing and fairly easy to make. It might very well be applied to measurements of what goes on during the explosion in an engine cylinder.

MR. MIDGLEY:—I think we do not differ greatly in opinion, except as to the possible value of continued spectroscopic work. I think Mr. Sharp has repeated some of the limitations I put upon it. On the other hand, except from Henry Lord and Prof. A. Trowbridge, we have received nothing but discouragement from people really qualified to carry on the work. Therefore I ask the members to get behind and push forward instead of getting in front and pushing backward.

E. R. HEWITT:—Has it occurred to Mr. Midgley to sensitize his film in lines, each line being sensitive to a certain wave-length? A number of years ago Captain Abney took photographs of a pot of boiling water in a room at a distance of 10 ft. without difficulty, and the films have been sensitized from the temperature of boiling water. It would be possible to sensitize a film in a series of lines, each one being sensitive to some definite temperature or some particular wave-length. In that way, if photographs were taken as has been described, one would get a series of bright spots on the film sensitized for the particular wave-length. One would be able to form a very good opinion without the use of any other instrument, lenses or prisms. I think that much more accurate results could be obtained in this way.

MR. MIDGLEY:—I shall be glad to try if Mr. Hewitt will send me the references as to where I can find out just how to do it.

A MEMBER:—We ought not to be discouraged because the displacement is small. I discovered a man in Chicago who could divide a wave-length of green light into 1,000,000 parts. I have seen those parts and counted some of them. So I think Mr. Midgley is right. Possibly it may be necessary to call in someone else to help, but he is well within the range of measurement, I think, judging also from the wonderful results Dr. Michaelson has achieved in measuring the flexibility of the earth's crust and in his complete investigations of the tides in the region of the Great Lakes.

MR. MIDGLEY:—There should be no difficulty in measuring 0.1 Angstrom unit. The pressure should be of such magnitude that it would give a shift of 3 Angstrom units. Consequently, 0.1 Angstrom unit only represents an error of about 3 per cent.

J. H. HUNT:—I can add nothing to the data as applied to the gasoline engine, but it is worthwhile to call attention to the fact that Messrs. Midgley and Gilkey are really starting exploration work.

Scientists can raise questions as to the possibilities of this work. Two classes of scientists are doing a great deal of work with spectroscopy. One group is using spectroscopic methods to determine phenomena on the sun and even on the very distant stars, in the case of which no other methods are available. Another group has been studying the relations between the wave-lengths of light emitted under various circumstances by substances under test and attempting to get information from the relation of these wave-lengths that will tell them what the actual structure may be in the atom emitting the light.

There has not been very much work in spectroscopy along lines directly applying to engineering. It will probably result that, as the work is carried farther, Mr. Midgley and others working along this line can take definite questions to the experts in spectroscopy for them to work out. Many of these experts would be just as willing to work on a question in spectroscopy that would have engineering applications as on problems they are attacking. There are many able workers in so-called pure science who are studying problems having no immediate application. They would gladly gather information that we could use at once if the need of solving these special problems were brought to their attention. Therefore, I think that this exceedingly interesting work will be very valuable in the end, because it will bring to some of these scientists questions that they have not thought of, the answers to which would be of very great assistance to us.

Some of Mr. Midgley's work on the anti-knock compounds raised questions in chemistry that the chemists had not answered but were of great interest to the chemists. Encouraging workers in pure science to give more attention to some of the problems having a direct relation to everyday engineering work will contribute greatly to our progress.

CHROME-MOLYBDENUM-STEEL APPLICATIONS FROM THE CONSUMER'S VIEWPOINT

BY C. N. DAWE

THIS paper gives a plain statement of facts regarding the application of chrome-molybdenum steels, as noted by a large manufacturer of automobiles, more than 2000 tons of molybdenum steels of various compositions having been consumed in tests and the manufacture of all the important steel parts such as transmission and differential gears, rear-axle shafts, transmission shafts, steering-knuckles, steering-knuckle pins, and the like. The data of an extensive set of physical tests are given, comparing medium-carbon, chrome-molybdenum, chrome-vanadium, chrome-nickel and chrome steels, and the results are expressed by means

of a merit index, taking into consideration yield-point, ultimate-strength, elongation and reduction of area.

The case-hardening grades of steel are discussed, difficulties that were encountered in the use of the low-carbon chrome-molybdenum steel in this connection being specified. The possibilities of nickel-molybdenum steel for case-hardening purposes are considered, the results so far indicating that it is a strong rival of low chrome-nickel steel, which is considered the best steel for commercial case-hardening at present. [Printed in the January, 1922, issue of THE JOURNAL.]

THE DISCUSSION

R. M. BIRD:—In the comparative tests shown Mr. Dawe refers to chrome-nickel steel; the analysis that he gives corresponds practically to that of steels in the No. 3100 grade. Have any comparisons been made on the basis of the No. 3200 grade?

C. N. DAWE:—No, not in this particular test.

R. H. SHERRY:—Has Mr. Dawe tried any chrome-nickel steel of about 0.35 per cent carbon?

MR. DAWE:—No, not in this test, although I have some information on the subject.

MR. SHERRY:—I ask that because, as a rule, if one runs below 0.30 per cent carbon the results may seem to be somewhat misleading in regard to the merit index. It might be worthwhile to carry this farther with 0.35-per cent carbon chrome-nickel steel for comparison.

MR. DAWE:—It would be interesting to run a series of tests along the same lines as given in this paper on the different compositions of chrome-nickel steels. However, I have applied this formula to a number of combinations of physical properties obtained by others with chrome-nickel steels of both the No. 3200 and the No. 3300 series and the results obtained corroborate certain conclusions that can be made from the tests given here. These results indicate that, as a general rule, the merit-index number increases and decreases inversely with the Brinell hardness. To compare one steel with another, the merit-index figures, apparently, must be compared where the hardness of both is relatively the same. As an illustration, I have a merit-index figure of a chrome-nickel steel of approximately the same composition as the chrome-nickel steel used in this test, except that the carbon is 0.35 per cent and the Brinell-hardness number is 207. This should be compared to the last one given in Table 5, which has a Brinell-hardness number average of 209. The 0.35-per cent carbon-steel has a merit index of 95 as against 73.4, which would be expected because of the higher carbon. Likewise, I have a merit-index number of 88.3 for a 0.24-per cent carbon, 3.19-per cent nickel and a 1.01-per cent chrome steel of a Brinell-hardness number of 248; while the merit-index number of the chrome-nickel steel used in this test, of which the average Brinell-hardness number is 254 as appears on line No. 8 in Table 3, is 65.2. According to our theory, the greater merit-index number is due to the superior analysis.

A peculiarity I have noticed is that a No. 3300 series steel in the neighborhood of 0.25 per cent carbon shows a much greater merit-index figure than a No. 3300 series steel in the neighborhood of 0.35 to 0.40 per cent carbon, provided both have approximately the same Brinell hardness. The former is the only one that approaches the results obtained with chrome-molybdenum or chrome-vanadium steels in this test. If this merit-index formula means anything, does not the above suggest that we may have overlooked something?

MR. SHERRY:—We made a few tests on these steels and found approximately that, if we had about 0.28 to 0.30 per cent molybdenum, the results compared very closely with those of the chrome-nickel steel of about 0.35 per cent carbon. The nickel and chromium contents were a little higher in that sample. It might be well to check that also, because this is at the lower end of that No. 3135 series.

MR. DAWE:—Have you ever applied this formula to your results?

MR. SHERRY:—No. Some other factors do not seem to be covered by the merit index. There are steels which had a rather poor merit index that gave good results under actual production tests; and steels that had a high merit index that gave poor results.

J. H. NELSON:—The Brinell-hardness value of these various steels would add considerable value to this tabulation. Surely the Brinell hardness of chrome-molybdenum steel, having an elastic-limit of 146,000 lb. per sq. in. is hardly to be compared with that of steel having an elastic-limit of 100,000 lb. per sq. in.

In furnishing forgings to customers we are held very closely to Brinell-hardness tests. It seems to me that a fair comparison would be between steels having the same Brinell hardness; that is, it matters little what we are able to produce in a steel with regard to physical properties if the hardness is such that machining is out of the question. We must deal entirely with steels that will give production results. Brinell hardness is a very important factor in the consideration of these results. We agree with Mr. Sherry in regard to the particular chrome-nickel steels selected; the carbon and chromium contents are low. If the chromium-content of this steel were raised, as well as the carbon-content, there would be no difficulty in producing the same result as shown with the chrome-molybdenum steel and, as we find, the same Brinell hardness; but the Brinell hardness which we are allowed to use is such that the full physical properties that can be developed are not available.

With regard to the merit index, the results presented show very clearly that this merit-index equation is far from satisfactory. For instance, the first chrome-molybdenum steel given in Table 2 of Mr. Dawe's paper, having an elastic-limit of 146,850 lb. per sq. in. combined with 55.6 per cent reduction in area, is not so good a steel according to the merit index as the first one reported in Table 5, which is a chrome-molybdenum steel having an elastic-limit of 108,500 lb. per sq. in. and a 62-per cent reduction in area. To my mind the steel given in Table 2 is far superior. I agree thoroughly with Mr. Dawe that reduction in area is a very good measure of ductility. A steel such as that cited in Table 2 is in better condition to resist stress or do work than the latter steel having a merit index of 85. This clearly shows that the merit-index equation should be used with caution.

I cannot agree with Mr. Dawe that there is any definite relation between contraction and impact value. That is dependent entirely upon the particular steel with which one is dealing. One may have a chrome-nickel steel of high elastic-limit and high contraction, but exceedingly low impact value; another steel of identical analysis may produce identical physical properties, with the exception of impact. In other words, one may have a steel of 55 or 60-per cent contraction that will give from 35 to 60 ft-lb. impact value. Another steel may have identical physical properties but an impact value of about 4 ft-lb.; that is, the relation between contraction and impact depends entirely upon the kind of steel with which one is dealing.

MR. DAWE:—I have tried to present some facts regarding chrome-molybdenum steel. The chrome-nickel steel

listed on the last line of Table 3 of my paper has a merit index of 65.2; the average Brinell hardness of all those tests was 234. The chrome-molybdenum steel above has a merit index of 88.3; the average Brinell hardness was 244. These are not far apart, of course, and two or three other combinations might be given.

Regarding Mr. Nelson's opinion that the chrome-molybdenum steel in Table 2 having a 146,850-lb. per sq. in. elastic-limit, is better than that having 108,500-lb. per sq. in. elastic-limit, in Table 5, the merit index of the latter, 85.0, is considerably over the merit index of the former, 72.8. Apparently the ratio between the toughness and the elastic-limit is greater in one case than in the other.

MR. NELSON:—Table 2 of Mr. Dawe's paper does not show a single reduction in area below 54. If we compare the merit index of this table with that of Table 5, we find that the latter is higher. However, I prefer every steel given in Table 2 to those of Table 5. It seems to me that this equation must be improved before it will give a real merit index of steels.

MR. DAWE:—Regarding impact, my conclusions are based upon a test of chrome-nickel steel and, as I have said, may not have been carried far enough, although the conclusions are made on pretty good authority. The impact tests follow the reduction-of-area values pretty closely. When the reduction of area increases, the impact value increases, and vice versa. I made some impact tests on case-hardened steel that indicate the above conclusions.

MR. NELSON:—Mr. Dawe and I agree, but we are talking about different things. He is speaking, I believe, of tests he made on steel from the same melt. Undoubtedly, in the case of steel from the same melt, the greater the ductility is, the greater the impact values. I referred to different heats.

We have made many hundreds of thousands of impact tests and not yet arrived at any conclusion as to their meaning, but we have found that the method of making the steel is a large factor in connection with impact value; that is, we have yet to find a steel made by a basic process, speaking now of alloy-steels and particularly of chrome-nickel steels, that has a poor impact value. In like manner it is rare to find a steel made by the acid process that has a good impact value. The same is true of carbon-steels, but in the reverse order; that is, we find, as a general rule, that we get better impact values with a 0.40 to 0.50-per cent carbon-steel made by the acid process, while the corresponding values with basic-process steel are decidedly erratic and poor. One may get an impact value on a 0.40 to 0.50-per cent carbon-steel as high as 80 ft-lb. but, as a general rule, it will be about 8 ft-lb.; consequently, we have not been able to correlate the impact values with ductility or any other physical property.

G. L. NORRIS:—Will Mr. Dawe tell us whether the tests on the steels reported in the tables were made on rolled bars in all cases, as in Table 3 for 1-in. rounds?

MR. DAWE:—Except for a few tests all the 2-in. square bars were rolled; the others were forged from 2-in. bars.

MR. NORRIS:—How is it done?

MR. DAWE:—On a small hammer.

MR. NORRIS:—What are the different methods of making molybdenum steel? Is it ever made with calcium-molybdenate? Does that give the same results as ferro-molybdenum?

MR. DAWE:—I cannot answer the last questions. All the steel we used was made from ferromolybdenum.

GEORGE WILLIAM SARGENT:—Mr. Dawe's paper presents the features of molybdenum steels from a viewpoint that is very interesting to the automotive engineer and, in fact, to any engineer engaged in the manufacture of equipment and machines in which steel is employed. He represents the consumer of a very large quantity of steel. The automobile is subjected to very severe service; it taxes to the utmost many of the steel parts comprising it. Hence, the suitability of the molybdenum steels for automobile parts is a strong recommendation. The old saying regarding the pudding is the basis of his viewpoint. He has not only eaten, but taken a second helping, perhaps several more. I refer to the tables showing the quality values of the chrome-molybdenum steels in comparison with other alloy-steels and their similarity to quality-value tables previously published.

I call attention to Mr. Dawe's statement regarding the small demand for a steel requiring proper and satisfactory heat-treatment through a wide range of temperature. His position is well taken in view of the modern instruments for controlling heat-treatment. But the automobile builder does not handle large pieces which require huge furnaces and quenching baths, where a wide range of temperature becomes very important. We are at the threshold of the adoption of alloy-steels for huge structures, such as bridges, the parts of which are 60 ft. or more in length and correspondingly large in other dimensions. Furnaces possessing a uniform temperature throughout and capable of holding such pieces are costly and difficult to build. In such instances molybdenum alloy-steels show to advantage through having a wide heat-treating temperature range. We have found this feature most beneficial in making large rolling-mill rolls, gun tubes and other forgings and castings. Furthermore, the saying, "There's many a slip 'twixt the cup and the lip," is all too true in heat-treated-steel products, and the wider heat-treating ranges permissible with these molybdenum alloy-steels, as a whole, add to the assurance of the quality of the product.

By "cracker shearing" I presume Mr. Dawe refers to the shearing employed on drop forgings where a trimmer die is used that leaves the forging with a flange or flash, or in some cases a tear extending a slight distance into the body of the forging. Such pieces are liable to develop hardening cracks, particularly if the steel contains hardening elements to such a degree that it hardens too intensely. Hence, as Mr. Dawe points out, there is need to use steel containing limited amounts of carbon, chromium and molybdenum, as well as manganese, phosphorus and sulphur, in such forgings as are to be heat-treated.

Referring to case-hardening grades, chrome-molybdenum steel gives a very tough and strong core with a very hard case when heated to 926 deg. cent. (1702 deg. fahr.) and quenched in oil after having been carbonized at 900 to 926 deg. cent. (1652 to 1702 deg. fahr.). When drawn at 400 deg. fahr., this toughness is increased. The high temperature employed to quench increases warpage and, as Mr. Dawe points out, is commercially impractical with pieces of irregular shape and non-uniform section, such as gears. It is presumed that this is partly on account of the injurious effect of such high temperatures upon the metal pots containing the lead or salt baths.

Nickel lowers the Ac_3 point of steel and this, no doubt, caused Mr. Dawe to turn to nickel-molybdenum steel in order to preserve the good features of molybdenum. I should like to see what results would follow the use of manganese instead of nickel. Its effect on the Ac_3 point is very marked. Molybdenum has a counter effect to that

of manganese, the latter tending to narrow the heat-treatment range and increase grain-growth, producing brittleness. Such steel, in view of the lower cost of manganese, would be less expensive. I suggest that the manganese content be around 1.20 per cent and that the quenching medium be oil or warm water.

Mr. Dawe calls attention to the better case-hardening properties of steel containing molybdenum, and states that a rolled bar carburized and quenched in oil will show greater hardness than any other hot-rolled steel so treated. Might not this characteristic of the molybdenum steels be due in part to their greater freedom from scale? He notes that they forge easily and that the scale formed breaks away readily. In hot-rolling also this tendency of molybdenum steel to free itself from scale is marked, and cleaner steel would carburize more readily. The greater hardness of molybdenum chrome or manganese carbide would account for the further difference in hardness noted. This feature offers a field for the production of hot-rolled, case-carburized and hardened parts with little or no grinding or further work. Such parts, when used in crushers, dredges, conveying machinery of certain types and coal-cutting machines, and in fact any hot-rolled steel products where service conditions require a hard surface to resist abrasion, would be improved if made of molybdenum steel, case-carburized and quenched.

J. D. CUTTER:—I think that Mr. Dawe did not intend to convey the idea that this merit-index formula could be used as an absolute standard of merit to be applied to comparing any two sets of tests without regard to the conditions under which the tests were made. Mr. Nelson called attention to the fact that the results cited at the top of Table 2 would be preferred to those at the top of Table 5, though the merit index of the former is lower than that of the latter. I think that all will agree with him in that statement. The tests in Table 5 were made on 2-in. square sections and those in Table 2 on ⅞-in. sections. The merit index cannot be used to compare sets of tests made under such dissimilar conditions. However, it offers figures that are useful in comparing different steels when broad averages are taken. I believe that in the average curves presented by Mr. Dawe in Fig. 1 of his paper, the sections are all average and the drawing temperatures averaged in the curves in Fig. 2, so the curves have much merit as a basis of comparison.

So far as my experience goes, calcium molybdenate can be used in the manufacture of molybdenum steel with as good results as with ferromolybdenum. I think that no steel using calcium molybdenate has been made in the last 18 months.

MR. SHERRY:—I think if Mr. Dawe would add to his paper the Brinell-hardness numbers that he obtained with these individual tests, they would be of considerable value. We found there was considerable variation in the Brinell hardness.

MR. DAWE:—The average Brinell hardness for each set of tests is appended herewith, the table numbers corresponding, respectively, with those given in the paper.

AVERAGE BRINELL-HARDNESS VALUES

Grade of Steel	Table 2	Table 3	Table 4	Table 5
Chrome Molybdenum	349	336	343	288
Chrome Vanadium	335	324	307	286
Chrome	294	291	281	256
Chrome Nickel	286	281	284	256
Chrome Molybdenum	299	277	295	281
Chrome Vanadium	294	289	302	289
Chrome	258	243	242	220
Chrome Nickel	270	254	255	220
Chrome Molybdenum	262	244	247	263
Chrome Vanadium	283	236	268	259
Chrome	229	238	229	207
Chrome Nickel	233	234	229	209

DEVELOPING A METHOD FOR TESTING BRAKE-LININGS

BY S. VON AMMON

AS a result of the general policy of the Motor Transport Corps to standardize the materials used for automotive vehicles for Army Service, in cooperation with the Bureau of Standards, the Society of Automotive Engineers and the automotive industry, the Bureau of Standards has been engaged for some time in developing a standard method for testing brake-linings. While the work is not completed, much information has been gained. This paper reports the progress of the work.

The equipment developed and the methods used for both main and supplementary tests are described. Information is given regarding the coefficient of friction, as influenced by various factors. The endurance test, showing the comparative behavior of linings under conditions similar to those of severe service, is believed to be satisfactory as developed. Further work is necessary before recommending the conditions for the other test, intended to determine the relative endurance under ordinary or light service. In work done so far with a cooled drum and over a very wide range of power absorption and speed, difficulties arising from the accumulation in the lining of particles of steel cut from the drum have persisted. Supplementary tests covering the tendency of a lining to stick when brakes are left applied on a hot drum, and to ascertain the relative absorption of oil and water, are described. The influence of oil and water on the coefficient of friction is shown.—[Printed in the March, 1922, issue of THE JOURNAL.]

THE DISCUSSION

A. M. WOLF:—Will Mr. von Ammon state what was observed in regard to the brake-drums insofar as abrasion and distortion are concerned? Was the condition of the drum surface the same at the beginning of each test? There is no doubt that a difference might result in the life of a brake, depending on the type and material of the drum. The ordinary soft-steel stamped drum would be at a disadvantage compared with some of the machined drop-forged types.

S. VON AMMON:—The drum was ground after each test to establish a smooth working surface for the next test. It is believed that the type and material of the drum may have a marked influence on the life of the lining; and, to make results comparable, the drums used should be closely similar. This consideration led to the adoption of the standard low-carbon-steel drum used. It is planned to investigate later the influence of the drum material.

We have not been able to investigate the quality of

asbestos or impregnating compounds. It has been necessary so far to confine our work to developing the method of testing.

Any operator, using for the first time a new type of apparatus, is apt to encounter difficulties. The following suggestions may prove helpful in anticipating them. To reduce vibration the brake-drum should run true and its surface should be smooth. Before using a new drum the scale should be removed and after each test the drum should be ground to remove any deposit and to provide a smooth surface for the next test. This grinding we have done by hand with a piece of carborundum, using proper precautions. The samples should be mounted in their proper positions in the flexible steel shoes, and have good contact.

To reduce as much as possible the influence of uneven mounting of the sample on the wear factor, we subjected the samples to an initial pressure while the drum was stationary by applying a 150-lb. tension between the pressure-arms. This was measured by a spring-balance. After a few minutes this pressure was released; then a 50-lb. pressure was applied. The distance C between the marks on the pressure-arms was measured and, after again releasing the pressure, the test began. Other measurements of C were made after each stop, the pressure during the run being entirely released and then increased again to 50 lb. The daily test was discontinued when, near the regular stopping time, a wear of $C_1 - C_2 = 1.25$ in., or approximately that, indicated. To secure accurate data on the minimum value of the coefficient of friction, readings should be taken at least every 5 min. during the early part of the run. Later, when variations are small and operation is smooth, readings at 10-min. intervals are usually sufficient. It is well to check the spring-balance occasionally and, when the wear of the pinion and rack affects the readings, to replace them by spares, which may be obtained from the maker.

As a few machines were built with torque-arms 35 in. in length in accordance with the Bureau's first equipment, as against the later 28-in. arm, it may be well to point out that, for 6 hp. at 600 r.p.m., the force acting at the end of the torque-arm designated by W in the paper should be 18 lb. for a 35-in. arm and 22.5 lb. for a 28-in. arm.

The reduced heat capacity of a much worn drum tends to shorten materially the life of the lining and it is therefore recommended that a test drum be discarded when the rim has been worn to two-thirds its original thickness, and preferably sooner.

MANUFACTURE AND APPLICATION OF AUTOMOBILE VARNISHES AND PAINTS

BY L. VALENTINE PULSIFER

DIVIDING the ability of an automobile finish to remain new into the elements of proper quality of the materials, engineering of application systems, methods of application and care of the finish, the author states that the responsibility for them rests jointly upon the manufacturer of the varnishes and paints, the builder of the automobile and the owner of the finished product. Five basic materials that are necessary in automobile painting are specified and discussed.

Engineering systems of application and the actual methods of application are treated in some detail, inclusive of drying, and of surfacing or rubbing. The care of the finish is important and the precautions necessary in this regard are outlined. The paper deals with the application and not the manufacture of the different varnishes and paints that are mentioned. —[Printed in the January, 1922, issue of THE JOURNAL.]

THE DISCUSSION

L. V. PULSIFER:—I will supplement my paper by presenting some additional data on the materials used in varnish manufacture, the engineering of an automobile painting system and methods of paint and varnish application.

To many persons not conversant with the details of scientific varnish manufacture the purchase of a barrel of oil, a case of gum, a keg or two of driers and a drum of spirits of turpentine or turpentine substitute and their haphazard combination in a copper kettle probably constitute their idea of how varnish is made. The condition indicated existed in the old days when the itinerant varnish-maker went from one carriage-shop to another with his little stock of materials and primitive apparatus and made several months' stock of varnish on the job.

As an illustration of how the industry has progressed from its primitive beginning, I will discuss three sets of materials.

The first set includes typical driers and thinners: namely, litharge, red lead, cobalt acetate and manganese borate, compounds of the three metals commonly used as driers or catalyzers for oleo-resinous varnishes; also, redistilled spirits of turpentine and a highly refined special fraction of volatile mineral hydrocarbon used at times in conjunction with spirits of turpentine to obtain differing rates of thinner evaporation.

The second set includes six typical gum resins that are used in varnish manufacture. These range from Batavia Damar, that is used in making varnish which, in conjunction with zinc oxide, produces the whitest of interior enamels, to Egyptian asphaltum, the highest grade of asphaltum in use in the manufacture of various types of black japans. Other typical examples are Zanzibar gum which 50 to 75 years ago was the standard for high-grade varnish, Kauri gum, black Kauri gum and Congo gum, the last being a fossil resin from the region of the Congo in Africa.

The third set includes eight examples. First comes the flaxseed as it reaches the oil refiner; before dirt, straw, foreign seeds and the like have been removed. The second is the raw linseed-oil as it reaches the varnish manufacturer, still cloudy from suspended "foots." The third stage is the same raw oil after it has been settled clear in aging tanks, a process requiring several months. This settled oil is then refined by using either acids or alkalis, depending upon what use is to be made of the oil. Fourth comes the refined oil. The refining process is necessary to remove the "break," such as the mucilaginous matter and various earth salts, that would otherwise become separated when the oil was heated for conversion into varnish oil. The fifth step in the process is this refined oil after it has been bleached by rapidly heating it to about 560 deg. fahr. After other impurities have been removed by chilling the oil to 20 deg. fahr. it is known as bleached oil. The oil is bodied by again

heating it to about 560 deg. fahr. and holding it at that temperature for from 5 to 6 hr. The oil has then about the viscosity of chemically pure glycerine, and is ready to be made into varnish. A quicker and cheaper process of bodying the oil, by blowing air through it while hot but using a much lower temperature than 560 deg. fahr., produces what is known as "blown oil." While this process saves time and cuts down the "cooking loss" on the oil to almost nothing, its use in varnishes tends to cause premature deadening or dulling. The final step of the process gives the completed wearing body varnish, the time consumed in its production from the arrival of the raw linseed-oil at the plant to the final testing of the finished product averaging at least a year. When it is considered that the oil is tested in the laboratory, both chemically and physically, on its arrival at the plant and at each subsequent stage of its treatment, the above outline will indicate the work necessary to be done on only one of the raw materials entering into the production of high-grade varnish.

The importance of the above procedure was illustrated in a panel finished with two body varnishes identical in formula, process and material, with the single exception that one contained an oil properly refined and treated and the other an oil refined in such a manner that the flowing qualities of the oil were destroyed. This panel was finished in a vertical position by working and brushing these two varnishes in exactly the same manner and finishing with a horizontal brush-stroke. The varnish made with the properly treated oil flowed out to a smooth even surface; but the other varnish presented a series of bad "sags" or "curtains" and in places showed signs of "silking."

A painting system can bring out inherent defects of engineering by showing the relations of the elasticity of the various coats to each other and to the completed system.

In Figs. 1 and 2 each coat is represented by a hori zontal line and the elasticities of the various materials by the length of the lines. The ends of these lines are connected by a line representing the "elasticity curve" of the system. The curve should be a smooth one as is brought out in Fig. 1, which shows that one should start with the primer and build *down* in elasticity to the flat color, and, starting at the flat color, build *up* gradually in elasticity to the finishing coat. If the materials are right, such an elasticity curve will indicate the durability and safety of the completed job.

Fig. 2 shows a system where the engineering was not properly done and was an actual case that we investigated. The layout was plotted approximately from the system in use. There was a good primer, the "half-and-half" and roughstuff were right, but the combination used for the flat color was too elastic; it was much more elastic than any of the rubbing coats that followed. Ordinary japan color was used and to it was added a considerable amount of an elastic sealer as a binder. This sealer was a material of high elasticity and enough of it was added to the flat color to make the flat color two or three times as elastic as the subsequent coats of rubbing varnish. The result was that the system pulled apart, due to the too elastic intermediate material. The finishing coat was torn and went to pieces in about one-third the normal time that would have been required if the flat color coat had been of proper elasticity. With a "sandwich" of that sort, an elastic coat between two brittle ones, one can upset the curve anywhere and cause trouble. If the rubbing coats are reversed, placing the least elastic one on top, trouble is sure to appear in the

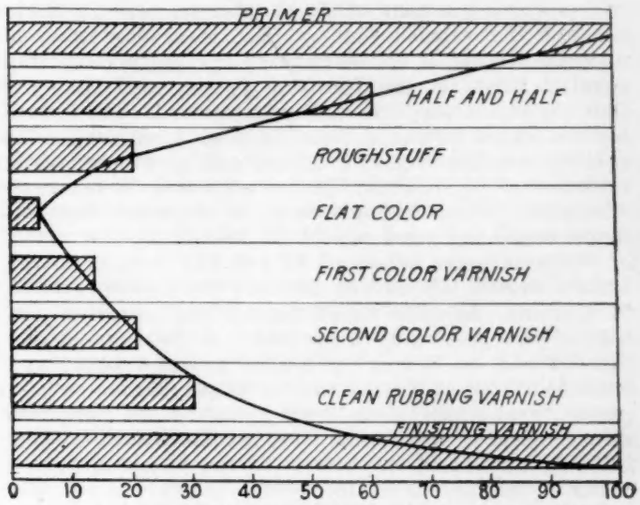

FIG. 1—PROPER CORELATION OF ELASTICITY IN THE VARIOUS COATS IS SHOWN IN THIS INSTANCE

premature breakdown of the finish. Figs. 1 and 2 show a study of the engineering layout of a foolproof painting system and the proper information as to the elasticity of the various coats.

The third subject is one of considerable importance at present: namely, the comparative results obtained by brushing on and by flow-coating the rubbing and finishing coats. To compare results and show in figures exactly what takes place in the application of varnish by these two methods, the four glass panels shown in Fig. 3, each 2 ft. square, were prepared. Two of these panels were varnished with automobile rubbing-varnish; one by flow-coating the rubbing varnish on, and the other by brushing, by a good brush-hand. The other two panels were varnished with wearing body-varnish; one by flow-coating and the other by brushing. Glass panels were used in order to measure the thickness of the dried varnish film, which was done in the following manner. A small cut was made in the varnish, going through to the surface of the glass underneath. The panel was then placed under a microscope, using a lens magnifying about 500 diameters. First, the surface of the varnish and then the surface of the glass were brought into focus. The difference between these two positions, read on the

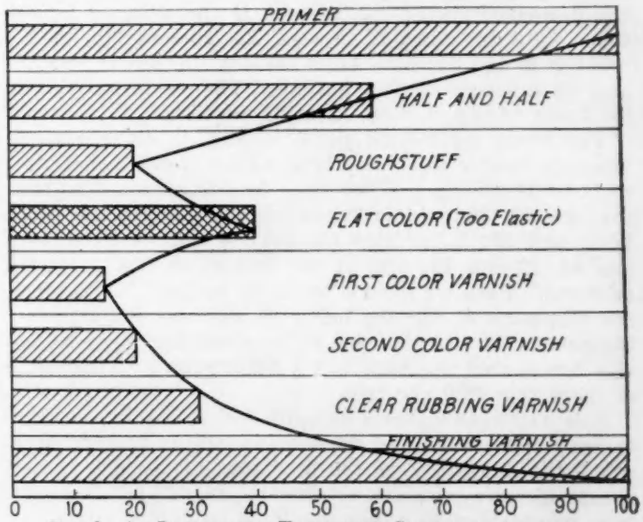

FIG. 2—AN INCORRECTLY ENGINEERED SYSTEM OF AUTOMOBILE PAINTING

micrometer adjustment of the microscope, gave the thickness of the varnish film at that particular point in microms. When it is remembered that 25 microms are equal to 0.001 in., the following figures read from the four glass panels will be readily understood. The figures for the upper left-hand panel in Fig. 3, on which the rubbing varnish was flow-coated, are as follows: The thickness at ½ in. from the top edge was 18 microms. About 3 in. from the top a series of thickness measurements across the panel ran 22, 23, 23 and 23; the middle of the panel gave values of 41 and 38; 3 in. from the bottom showed 53, 54 and 52; and the bottom showed 56 microms. In other words, the rubbing varnish at the bottom was practically three times as thick as at the top. It was too thin at the top for proper rubbing and much too thick at the bottom for proper hardening. If two or three similar coats were flowed on and the panel were finished in the normal way, the finish would begin to "alligator" from the bottom of the panel up; great cracks would appear at the bottom where the rubbing varnish was too thick. Not only does the thickness vary but, when one section of a panel is backed up with black, marked vertical "silks" will show in the rubbing varnish when the latter is flow-coated on.

In the upper right-hand panel, in Fig. 3, the rubbing varnish was brushed-on by a good brush-hand and measurements similar to those that were obtained on the previous panel are given. In this case the thickness ½ in. from the top is 25 microms, or about 0.001 in., which is a fairly normal thickness for a rubbing varnish. At 3 in. down it was 24, 23, 31 and 27 microms. In the middle it showed 25 and 26 microms; 3 in. from the bottom it showed 35, 35 and 34; and at the bottom edge where the last cross-stroke of the brush took place it was 32 microms. This gives a difference of from 25 to 32 microms between the top and the bottom, as against the difference in the flowed-on coated panel of from 18 to 56 microms.

The same results were obtained with finishing varnish. The micrometer measurements of a wearing body varnish flowed-on to a duplicate set of panels to the two just described read as follows. The lower left-hand panel in Fig. 3 showed 19 microms at the top. That is much too thin for suitable durability of the finishing coat and, unfortunately, it comes at the place of the hardest wear. In other words, the top of the panel gets the strongest sunlight, the worst hand abrasion and the like. Under the flow-coat method the varnish is the thinnest at the top. At 3 in. down it showed, 28, 29 and 30; in the middle, 49 and 49; 3 in. from the bottom, 56, 56 and 58; and 62 microms at the bottom. The variation in the thickness of film is from 19 to 62 microms.

The lower right-hand panel in Fig. 3, on which the wearing body varnish was brushed-on, shows a measurement of 32 microms at the top. At 3 in. down, the readings are 34, 36 and 38; in the middle, the film thickness is 41 and 40; 3 in. from the bottom the measurements are 44, 43 and 42; and at the bottom of the panel, 44 microms. Here we have a variation of only 12 microms, the minimum at the top being 32 and the maximum at the bottom 44. In the case of the finishing varnish that was flow-coated on, there was a difference of 43 microms, or more than 200 per cent.

It is well known that a uniform film is highly essential to both appearance and durability. Other things being equal, the durability of the final coat of varnish varies almost directly with its thickness. In other words, up to a point where it is not too thick to harden properly, its durability would vary almost directly with its thickness. If it is one-half as thick as it should be, it will be only one-half as durable. The fullness of the finish at the top of the panel will also be far below par, as the finishing varnish would have only about 50 per cent of its normal thickness at that point.

I hope that what I have said will tend to start a more scientific study of some of the problems I have outlined, on the part of the automobile builders, and that it will lead to greater cooperation between the makers of automobile finishing materials and those who use them.

W. C. KARNS:—From my experience, I think that not enough time is given to the ground coats for drying. The color coat, which is generally japan color, usually is applied as a base coat and it invariably causes trouble with the coats that are applied afterward. By eliminating the japan color coat, which I find can be done by using for ground work practically the same colors as one uses for the finished job, a more durable and a more substantial job of painting is produced in the end. Is it considered practicable to do that? I have done it with good results and watched it very carefully. I deem the flat color coat a trouble coat in most painting.

MR. PULSIFER:—There is confusion of terms as to what the color coat is. Some people speak of flat color, others of japan color, others of body color and others use different names. It is impossible to eliminate the color coats for the reason that the color varnish, as a rule, does not cover solid; but, the fewer the number of flat color coats put on, the better it is and, if the ground work can be made to serve the purpose of completely coloring the body so that the next coat will be the color varnish, possible trouble that might be caused by piling on too many coats of so-called japan color or ground color will be eliminated. Some of the old painting systems called for a coat of ground color and two coats of japan color, the ground color sometimes being a color of similar nature but poorer quality, followed by two coats of the highest grade of japan color, and then by color varnish and a clear rubbing varnish. With most colors at present these color coats can be cut down to one or at most two, and it is highly desirable to put on as few of these brittle coats as possible. In fact, in automobile painting it is just as risky, if not more so, to put on too many coats as it is to use too few. Some of the mistakes made in the early days of automobile painting were caused by piling on too many coats. The painters who worked on an automobile body did not realize the severe vibrations and strains it must withstand, these being much greater than those on any coach, carriage or railway car. An excessive thickness of paint is a detriment rather than an advantage; therefore, the number of coats has been reduced gradually since the early days of automobile body building. That tendency has been increased also by the adoption of metal as a base on which to paint. Mr. Karns is right in stating that the japan color coat or body color coat can be a source of trouble if it is put on too thick, or if too many coats of it are used, or if a coat is used that is not of the proper elasticity to correspond with the others. The color coats need not, however, be "trouble coats" if used properly.

A japan color coat should be so thin and so porous that the first coat of color will strike right through the japan color and bind it to the surfacer coats underneath. One can put a japan black on a piece of glass, for instance, and when it is dry put a coat of rubbing varnish over the black. If the glass is turned over, one will see the varnish come right through the black. That is an ideal condition for a japan color coat. If that coat is made too dense, too thick or too elastic, in attempting to make the job

durable, the "elasticity-curve" system is upset, as was shown in Fig. 2. Also, too many coats of the same elasticity upset a system of painting. There should be an easy curve from start to finish, as was indicated in Fig. 1.

C. S. RICKER:—How does Mr. Pulsifer determine the elasticity of these coats? How can we get a more economical dull finish such as is being talked of by a number of advertising men at present? My experience is that it costs more to put on a dull finish that will last than it does to produce a high-gloss finish of equal durability. What effect does the time of drying have on the elasticity and the durability? We are all interested in getting work through the factory as rapidly as possible. The problem of using artificial heat to assist the drying and the question of the proper degree of humidity are things we would like to know more about.

MR. PULSIFER:—Mr. Ricker has brought up several points that touch on the present rather unsatisfactory state of the finishing of automobiles. One is that the scientific men of the various automotive organizations pay very little attention to the finish. They have made very little attempt to find out why certain things were true and why certain other things were not true. Perhaps that has been a fault of the varnish manufacturers also. With regard to how the elasticity of these various coats is determined, the elasticity of pigmented coats depends first upon the elasticity of the liquid in which they are ground and second upon the amount and sometimes the character of the pigment that is combined with the so-called "wetting." The elasticity of each material can be determined readily by determining the elasticity of the wetting itself, and then by figuring the amount of pigment that is added to it. The pigment decreases the elasticity of an enamel or color coat, and the elasticity varies with the type of varnish used as a grinding medium, or wetting. As to the clear rubbing and finishing coats, it is possible to determine and state in definite figures the elasticity of any varnish by an elasticity reduction test that I devised a number of years ago. During the war this elasticity reduction test was used by the Government to determine whether a varnish was elastic enough for airplane use. The old method was to put two or three coats of varnish on a panel, place it out in the weather and find out how long it lasted before it cracked. That method had two objections. A considerable amount of time was required and the results varied at least 100 per cent on account of the difference in the weather. It is possible now to take an unknown varnish and by cutting down its elasticity by the addition of "run" Kauri gum, which is gum that has been melted and reduced to a condition to make varnish, to put it through a definite bending test on metal after it is dry. After determining the amount of run Kauri gum that one must add to bring the sample to the breaking point when the metal panel coated with it is bent-double rapidly, one can figure how elastic it was at the start. This test can be made readily on any of the varnishes used in making these automobile painting materials. An engineer who is interested in finding out the elasticity of the various coats would request the varnish maker to have the wetting of the primer, the roughstuff and the color varnish furnished to him as a clear liquid on which to make these tests. He gets the finishing varnish in a clear form, of course, and can make that test very readily in any good service laboratory.

ARTIFICIAL-HEAT DRYING

Very often a car is designed and a plant is built before painting or varnishing is considered as to paint-shop equipment, materials or production schedule, which shows no realization of the problems that will be encountered. One of the problems is that of the use of heat to speed the drying process. Heat properly used is highly desirable and, under modern schedules of production, it is absolutely necessary. In the case of materials such as we have been describing we do not mean an actual baking when we mention heat. Actual baking is used only in the case of such materials as fender blacks where a temperature of 350 to 450 deg. fahr. is used. Those are materials that would never air-dry and therefore must be baked. With materials such as we have been talking about the term "forced drying" is used; this means to increase the speed of drying by the addition of heat to the process. The heat aids the chemical action of the absorption of the oxygen, of course, and the drying is consequently more rapid. To make a general rule, one should use less heat with each succeeding coat, from the primer to the last coat of rubbing. The heat for the primer should not run over 150 to 175 deg. fahr., and it should grade down to 110 to 125 deg. fahr. for the last coat of rubbing. This heat must be applied at a proper rate; the jobs must not be coated and rushed right into a hot oven. There must be the proper percentage of moisture in the oven to prevent too rapid evaporation of the thinner and consequent surface hardening. Too high or too fast a heat causes surface or case-hardening. The ideal method is to run the jobs into an oven at the room temperature and then gradually raise the heat to the proper point. When it comes to the final flowing coat of body varnish, there is little to be gained by using forced drying. In fact, the only thing it does is to dim the luster. The body varnish can be made to dry free from dust somewhat more rapidly by using a temperature over

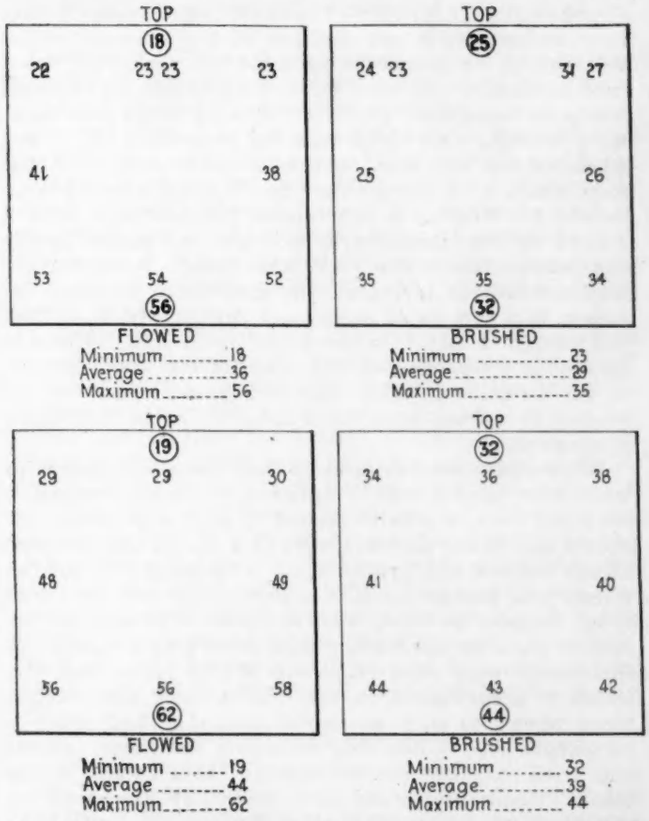

FIG. 3—THICKNESS OF COATS OF RUBBING VARNISH (AT THE TOP) AND (UNDERNEATH) WEARING BODY VARNISH WHEN FLOWED ON AND WHEN BRUSHED

100 deg. fahr., but the final hardening is scarcely assisted at all by forced drying. It is just about as dry 2 or 3 days after it is applied under good air-drying conditions as it would be if dried on the first day up to 110 or 120 deg. fahr. By heating it to that temperature one would get a thinner and less brilliant finish, on account of its great fluidity and the length of time that it remains in a fluid state after application to the body.

The subject of dull finish arises periodically. The desire for dull finish comes in waves and a wave seems to be at its crest now. There are two ways of producing a dull finish. One is to put so much pigment in the final coat that it dries flat. The particles of pigment stick up in the finish and cause it to have a flat appearance. The other method is to finish the car more or less in a normal way, and rub the final coat flat with pumice-stone flour and water. For durability, absence of spotting and good appearance the latter method is to be recommended. In that case a rather different type of varnish should be used on the body than is applied ordinarily. A varnish approaching more nearly in general nature to chassis varnish should be used for body finishing, because it takes a rub more quickly and better. Such varnishes can be made to have equal or greater durability than the body varnishes themselves, and their use avoids the common mistake of mixing rubbing varnish with the finishing when a job is to be rubbed flat. The method of putting enough pigment into the varnish results in a ready-made dull-finish but one that spots readily. This finish must be wiped off and polished continually with some sort of oil, to keep it looking well. Clear varnishes that will dry dull possess no durability.

Moisture Content of Wood[a]

Cornelius T. Myers:—For more than 2 years I have been engaged with one division of our industry in investigating the properties of wood; in particular, wood used in wheels. We discovered many things that seemed not to be recognized generally, even by those who make wood wheels. One thing was the protection of a wood wheel against moisture. It is essential to remember that wood changes its dimensions when it absorbs or gives up moisture. Wood is a hygroscopic material and, with a change in the atmospheric moisture, a change in the moisture-content of the wood takes place. Whenever the moisture-content increases, the dimensions of wood increase, to a very small extent and very slowly, it is true, but enough to affect its serviceability in time. There is very little increase along the grain of the wood, that is, in the direction in which the tree grows, but wood increases in a very substantial amount in the two other dimensions.

Wheels have been shipped by their manufacturers with what was called a coat of "primer." No one seemed to have any definite idea in regard to what a priming coat should be. When shippers were in a hurry, they shipped wheels without any primer at all. The man who got the wheels was just as much to blame as the man who sent them, because he often allowed them to remain in the rain or snow on the loading dock from 2 to 3 weeks. If the wheels were received in winter, he found that the moisture accumulated on the loading dock had swelled those wheels to such an extent that they had taken a permanent set. When they went into the warm factory and dried out, they became loose in the arch and in the felloe. Their usefulness was practically destroyed on account of the permanent set at the first swelling.

We conferred with the paint manufacturers to find out how they met this problem and what they had that actually would prevent the moisture from going into and coming out of wood; but they knew practically nothing about it, not even recognizing the matter as a problem. I believe that hardly a single paint manufacturer in the Country knew exactly what to do to wood to protect it against moisture.

We have conducted thousands of tests within the past year on various primers to ascertain how well they would protect wood. We outlined a test schedule and a number of paint manufacturers tried it out. After interviewing salesmen and others, including company officials, we let them test out what they said was the right paint. Invariably the regular products gave poor results when tested. One paint company tested three of its regular mixtures; one was very poor, another gave a better result and the third was still better. By modifying a few constituents this company then got results which were over five times as good as the previous average, insofar as moisture protection was concerned.

Then we turned to the Forest Products Laboratory, at Madison, Wis. Its representatives reported that they had investigated the subject at some length in connection with the protection of airplane propellers. When airplane propellers absorb moisture they do not always absorb it uniformly and become unbalanced. It was stated also that the absorption of a considerable amount of moisture by wood impairs its strength, which is a very important consideration. The Forest Products Laboratory has a great deal of information on wood, and is well equipped to make tests. Those interested agreed that this laboratory was a desirable place to carry on research work on wood preservation. There is a possibility of eliminating from 10 to 16 per cent of the $300,000,000 national paint bill per year. This applies not only to wheels, but to passenger-car and motor-truck bodies, body structures and frames. It includes also houses, floors, furniture and everything made of wood and subject to exposure to moisture. Better service can be secured from wood that is protected properly. I offer a resolution to the effect that the subject of wood-preservation research be brought before the Council of the Society and that the Society take action concerning it. The matter involves the conservation of our timber supply. It also opens up fields for the use of wood beyond any conception we have at present, airplane construction included. [The resolution offered by Mr. Myers was duly seconded, voted upon and carried.]

Mr. Pulsifer:—During the war this subject of moisture absorption through various types of paint and varnish films was deemed very important, especially by the Navy in its work in the construction of seaplanes. It was found that the hulls of seaplanes when exposed for a comparatively short time to rain or seawater sometimes became so water-logged that they could not fly due to the increased weight. Therefore, a series of experiments was conducted at the Forest Products Laboratory on the moisture absorption through various types of paint and varnish films. The Navy also took up the matter with the company I represent. I spent almost 2 years in research work on the absorption and dissemination of moisture through various types of paint and varnish films. We have records covering hundreds of experiments along those lines, and I would be very glad to have Mr. Myers see them. They proved that, with similar types of paint, one could obtain differences in both absorption and dissemination up to 400 and 500 per cent. The best kinds of material to keep moisture out,

[a] This discussion is a combination of Mr. Myers' remarks at the Business and the Body-Engineering Sessions.

or keep it in, under various conditions, were investigated carefully and the results of this research work were turned over to the aircraft division of the Navy. I presume that anyone interested in the shrinkage or expansion of wood can have access to this information.

Some wheels are shipped with no surfacing; others are shipped with a coat of what might be called a primer; but, in most cases, the latter is about as waterproof as flour paste. The automobile builder who thinks at all of finish apparently thinks last of the finish of the wheels, and the poorest material is put on the wheels. No attempt is made to get undercoats that are elastic enough to stand up without cracking or flaking around the joints, or varnish that is waterproof enough to prevent the rapid penetration or dissemination of moisture into and out of the wood. Mr. Myers apparently did not consider the varnish manufacturers as a possible source of information in this investigation. He may have been barking up the wrong tree. Much information is already available along those lines.

P. W. STEINBECK:—What would the effect be on the finished job of using different makes of material, such as the primer, roughstuff, varnish and finishing varnish, for each individual coat?

MR. PULSIFER:—The result might be a perfectly good job or a thoroughly bad one. A bad job can be produced when each material is right in itself but not suited to the others. Unless a firm has an engineering laboratory in which the various materials from the primer to finishing varnish can be tested, it is safer to rely upon some one manufacturer and get materials suited to one another. That is not the only way, but it is by far the safest way; otherwise the result might be either good or bad and no individual manufacturer could be held responsible. His material might be right and so might the materials of others, but they might be unsuited to one another.

MR. RICKER:—Did Mr. Pulsifer say that the body primer is not waterproof and will not protect the metal from rusting?

MR. PULSIFER:—That is the reverse of the condition that ought to exist. The porous and non-waterproof coat of the system should be the flat color. It is essential to have the primer not only tough and elastic but as waterproof as possible, especially where jobs are sometimes primed and allowed to stand for a considerable time in the priming coat. That is true, not only of wood wheels, but of metal bodies.

A MEMBER:—Are the results obtained by spraying varnish the same as those obtained by flow-coating? If not, can spraying be done satisfactorily?

MR. PULSIFER:—To my knowledge the spraying of color and finishing varnishes has never been done satisfactorily. There are certain objections to spraying. Undoubtedly one could spray a small panel and get a much more uniform coat than by flow-coating, but there is great difficulty in spraying a whole body with varnish. The spraying system, as a rule, is used in modern shops for every coat up to the color varnish. It is an excellent system for primers, roughstuff or surfaces and flat color. In fact, it is probably the best way to put those coats on. A man skilled with a spray gun probably can get fully as uniform a coat with a spray as the best brush-hand can with a brush, and all brush marks are eliminated. With the color and the finishing varnishes the spray has been frequently tried but without success. It may be used in what might be called a semi-gloss color-varnish that dries almost flat, but the results do not appear to be satisfactory with a real color-varnish. The best method of applying the real color-varnish, clear-rubbing and finishing-varnish coats is by using a skilled brush-hand.

MR. KARNS:—Would you advise the use of more than one coat of clear rubbing?

MR. PULSIFER:—In many shops one coat of clear rubbing-varnish would be an advance in the art. As a rule, the use of more than one coat of clear rubbing-varnish is not advisable. The last coat of color varnish may be what is commonly called a tint coat, containing very little of the color, and followed by a coat of clear rubbing-varnish, although in the case of some delicate tints, some color often is carried into the last rubbing coat. Blue has been mentioned as a popular color. It always is desirable to have a coat of clear rubbing-varnish on top of the blue, because blue is more chemically active with the varnish in the presence of moisture than almost any other color. If a coat of clear rubbing-varnish were put on top of the last blue coat, an appreciable increase in the durability of the finish would result, but with most colors it is not advisable even to consider more than one coat of clear rubbing-varnish. Two coats of color varnish, then two coats of clear rubbing-varnish, would build up to excess the brittle-varnish end of the painting system.

CHAIRMAN E. G. BUDD:—Would the same type of primer be used on aluminum-finished bodies as on steel?

MR. PULSIFER:—The same type can be used on aluminum and steel, but it is not a safe rule to say that it should be used always. It is not advisable to put a primer containing white lead as a pigment or as a hardener on aluminum. If white lead is in direct contact with metallic aluminum, rapid destruction of the aluminum will occur around moldings and the like, where the paint films become split and the moisture gets in. The primer coat for aluminum should not contain white lead. With that reservation, there is no reason the same type of primer should not be used on aluminum as on steel.

CHAIRMAN BUDD:—In the case of a steel body having substantial surfaces of solder, how would the same primer be affected by those two metals?

MR. PULSIFER:—If the soldering acid is removed and the surface is clean, the same primer will do for both. On bodies having a bad dent that has been built-up with a mass of solder, the paint over that particular surface may crack, due to the different rates of expansion and contraction of the solder and the steel under changes of temperature. Their coefficients of expansion differ greatly.

CHAIRMAN BUDD:—Is not the cleaning of the surface one of the most important questions in proper finishing?

MR. PULSIFER:—Unless the surface of the metal is cleaned properly, one cannot obtain a good finish, no matter how excellent the quality of the materials or the layout of the system may be. The surfaces must be absolutely clean, free from grease, rust, soldering acid or moisture of any sort. During rush work especially many jobs fail because of greasy fingermarks on the panels before priming; acid that works out under the moldings and other places; and rushing rusty and improperly cleaned bodies from the storage sheds to the priming department. Proper cleaning is very important. The first coat of material must be put on a surface perfectly prepared to receive it; otherwise trouble surely will result.

MR. KARNS:—How can the surface of galvanized iron be prepared to permit a durable job of painting?

MR. PULSIFER:—It is very difficult to paint galvanized iron but, if the surface is scrubbed thoroughly to remove

any acid and subsequently cleaned to remove any grease, there should be no difficulty in painting galvanized iron after a suitable primer has been applied. The usual difficulty with galvanized-iron painting is that the paint used will not cling to the galvanized iron. We have found that the same type of waterproof and elastic primer used successfully for ordinary automobile painting is very successful when used on galvanized iron, even for exterior buildings, provided the iron has been thoroughly scrubbed. The best acid remover is a good scrub with clean water. Benzol or something of that sort used afterward will remove the grease. Benzol is better than gasoline for washing all types of metal surface that are to be finished, because present-day gasoline contains a high percentage of material that does not evaporate readily. Benzol can be obtained at very little more expense than ordinary gasoline; it cleans more quickly and evaporates completely. Benzol is much more inflammable than gasoline and its one bad feature is that it increases the fire risk, which necessitates strict adherence to insurance regulations.

A MEMBER:—What system is recommended for painting light-tinted or gray jobs for export that remain boxed for months and then are opened and probably handled improperly? We have had considerable trouble due to discoloration, especially with grays, on portions of the body such as the hood and doors. These discolored in different degrees.

MR. PULSIFER:—If kept in the dark, any of the varnishes that are based on fixed vegetable oils will turn yellow to a certain extent and sometimes to a very great extent. The result is that a gray job, although finished in accordance with the very best practice, will turn yellow when put in the dark. It will turn yellow to a degree determined by the thickness of the last coat of clear varnish on top of the gray. With such a panel as that exhibited in the upper left-hand portion of Fig. 3, where the varnish is 18 microms thick at one end and 56 microms thick at the other, after the panel had been shut up in a crate for a number of months the discoloration would start almost with the original color of gray at the top, where the varnish was thin, and grade down nearly to a golden brown at the bottom, where the varnish was thickest. There is no way of avoiding this discoloration if the paint is to be kept in the dark. The only way I know of to avoid discoloration, which probably would be all right for export use, is to put on a last coat of a finishing enamel of the desired gray rather than a clear varnish. That would result in a very thin film of varnish being on top of the gray pigment and minimize the discoloration greatly. It also would make the discoloration uniform, because the same amount of varnish would be on top of the pigment over the entire surface of the work. I know of no other way to avoid discoloration except to store the painted bodies in the light; this, unfortunately, is not practicable in the case of automobiles crated for export and stored for any length of time. A gray enamel finish would help to remedy this condition materially.

MR. RICKER:—Is the discoloration of gray and blue the result of chemical action between the colored pigment and the varnish, or of the effect of light passing through different thicknesses of varnish?

MR. PULSIFER:—The fading of blue color is due to the fact that under the actinic rays of sunlight blue coloring matter is not as permanent as some other colors. The destruction of the finish over the blue is due to the chemical action of the blue pigment itself with the oils of the varnish; this causes the oils to harden and the varnish to become brittle. Probably many of us have seen work that perished in a very short time where a heavy percentage of blue was carried into the last coat of color varnish and then a thin coat of finishing varnish was flowed-on or brushed-on by one of the high-speed finishers. The blue pigment seemed to eat the varnish up, and that is exactly what it did. It is the same thing that would happen if the varnish were dropped into caustic soda. The blues we have been getting recently have been much less pure than those we were able to get before the war. They have not been washed so carefully and as a result contain various impurities. Everyone has met with the same trouble. The only safeguard is to bury that blue under a coat of clear rubbing-varnish before the finishing varnish is applied.

Gray pigment should have no destructive effect on the varnish except that, other things being equal, varnish will last longer over a dark color than over a light one. The reason is that the destruction of varnish is caused partly by the same chemical or actinic rays that I spoke of in connection with the fading of blue. When the varnish is over black or a very dark color, most of these rays are absorbed after passing through the varnish; but when varnish is over white or a very light color, part of these destructive rays are reflected back through the varnish. The result is that the clear varnish gets it coming and going and, for that reason, other things being equal, a finishing varnish will wear longer over a black surface than over a white one. Of course, the black surface becomes hotter than the white, but the heat developed is not enough to harm the varnish. It is the chemical rays, which are heatless, that destroy the varnish.

BODY SEATING-DIMENSIONS

BY GEORGE E. GODDARD

THE dimensions of automobile-body seats receive consideration with regard to the features that are conducive to comfort. A diagram is presented upon which the dimensions treated are indicated, and a tabulation of seat dimensions of 12 representative cars is included.

Comments are made upon the factors influencing seat dimensions, as well as recommendations regarding the different desirable dimensions. The considerations are inclusive of cushion height, depth and slope, leg-room and head-room, upholstery shape and softness of trimming, foot-rest and other control-element locations, factors influencing entrance and egress provisions, seat widths and advisable front and rear-compartment heights.

The author recommends the standardization of a range of locations for the different control elements. [Printed in the February, 1922, issue of THE JOURNAL.]

THE DISCUSSION

J. E. SCHIPPER:—The point that Mr. Goddard brought out about entering and leaving on the wheel side of the car is very important. He gives the distance between the steering-wheel and the left corner of the front-seat support as 9 in. I had occasion to measure a number of cars 4 or 5 years ago in which that dimension was as little as 7 in.; in many it was as much as 12 in. and on one car it was 14 in. It is an important dimension, particularly if a man has a passenger on his right side and

must enter and leave the car often. I know of one case where the spare tire, which was carried on the left side, prevented the full opening of the left front-door. Because of the small space and the restriction of the door opening, it practically was impossible to enter and leave the car on the left side.

In locating the controls, attention ought to be given to the position of the starting-switch pedal. In entering the left side of the car that pedal sometimes is accidentally stepped on, producing bad results when the engine is idling. Those two points are essential. Adequate hip-room is becoming more and more important also.

G. W. KERR:—In the front compartment, it is desirable to make sure that the operator has room to remove his foot from the clutch and place it on the toe-board. In some high-class cars there is practically no place for the operator's feet except on the pedal and the accelerator button. That is largely governed by the width of the dash and the arrangement of the lining of the inside of the cowl. The 2-in. slant of the seat stools seems to me rather under present practice and less than what is desirable. The general dimensions given by Mr. Goddard check very closely with what I believe to be proper measurements. I allow 7 in. under the steering-wheel to the top of the front cushion, grading the springs in the front cushion so that they will collapse to one-half of their ordinary length. That probably would let the passenger or driver sit at about the same height as is covered by Mr. Goddard's measurement.

The idea of using a standard-sized manikin as an aid to design is a good one but, rather than use one of average dimensions, I think the proper ones would be those of a man somewhat larger than the average, say 5 ft. 10 in. high. It has been found in the salesroom that, to insure the greatest amount of sales, the largest number of people must be accommodated. If average dimensions are used, anyone who is large will feel more or less uncomfortable, but there are ways of providing comfort for persons under the average size by changing the upholstery, placing extra cushions at the back and means of that kind.

G. C. BAKER:—We had an interesting experience some time ago. We found the manikin was too large, because it was not relaxed as the human body would be. I think it would be better to make it under rather than over the average size.

G. E. GODDARD:—Body engineers who have made it a practice to use manikins should submit their suggestions to the Passenger-Car-Body Division of the Standards Committee. It is a subject that I think is rightfully within its scope of work. We all know of the manikin that is made of cardboard and has few joints, but the human body does not have so much range of action as the joints of a manikin allow. There should be some kind of stop on the joints. There is need for a discussion and study of what the length of these joints should be. Every body engineer realizes the value to be obtained from a standardized manikin. The benefit is not so much in the comfort of the seat as in the softness or comfortable movement of the body, and has to do with dimensions and proportions. The manikin cannot be used very well to determine the shape of the back, but the average dimensions of the different joints can be obtained. This would at least prevent some of the short cushion-depths and short leg-length distances to the pedal that are still very common. I hope we can decide whether it is practicable to have a standard manikin. I think this subject should be cleared up.

JOSEPH LEDWINKA:—We have correct dimensions and use manikins having an average height of 5 ft. 9 in. with stops on the joints. We know exactly the movements of the ankle and knee and whether they are comfortable. By the use of these manikins we find it very easy to determine the seating capacity. It may be worked out by anyone on a one-quarter or a one-half-sized scale and made so that it will fit the regular predetermined dimensions.

L. C. HILL:—The engineering division of the Air Service at McCook Field has established a standardized manikin aviator around which airplane fuselages have been designed very successfully. I understand that the proportions of this manikin were obtained by measuring many pilots very carefully and averaging the resulting dimensions. The Air Service might be consulted in this regard and the conclusions that it has reached studied carefully with profitable results.

CHAIRMAN E. G. BUDD:—I think we need several manikins, an extra long one for leg measurements and one somewhat undersize for some other lengths such as cushion heights. It is very uncomfortable for a woman to sit in a seat which would be comfortable for a man of average or slightly more than average height. We need a large manikin to determine the distance between the steering-wheel and the front seat and one wearing a hat to determine the proper vertical measurement. This is an interesting subject and well worth the careful study of engineers.

THE PAST, PRESENT AND FUTURE OF THE MOTOR-OMNIBUS

BY WALTER JACKSON

AFTER outlining motor-bus history and relating how the introduction of mechanical and electrical propulsion on rails relegated horse-buses to the background, the author states that electric-railway construction is at a standstill and that the present demand for additional transportation facilities is being met by motor-buses that often are operated by individuals; presents railway statistics and comparative comment on jitneys, cross-country motor-buses and supplementary motor-bus service by electric-railway companies; and mentions many influential factors concerning motor-bus utilization.

Future motor-bus considerations include discussions of fare rates and operating expenses. The author believes that a comparison of the true cost of electric-railway versus motor-bus service has been obscured by certain factors having nothing to do with the engineering aspects of the situation, and that the economic fields of these services will be determined by the coordination of all mass transportation under one management instead of permitting indiscriminate and destructive competition. [Printed in the March, 1922, issue of THE JOURNAL.]

THE DISCUSSION

C. M. MANLY:—The use of motor-buses in suburban development bears a very different relation to the transportation problem from that of the street car or tram-

way. The value of suburban property has heretofore been based largely on transportation facilities and property owners are keenly interested in the subject. With the advent of motor-buses and the decline of the electric railway, I believe there will be a radical change in the situation. With motor-bus transportation a person who has property in a suburb cannot be at all sure that the transportation facilities at a later time will be the same either in location or adequacy as they are at the time he buys. On the other hand, a railway in operation involves a fixed investment and gives the land a fixed value. An illustration of the difficulties attending a fixed transportation investment was afforded by the situation in Toledo last year, where the street railway company was able to run its cars across the State line and thus withdraw part of the equipment; but the fixed investment resulted later in the return of this equipment to service in Toledo. On the other hand, with the motor-bus it is possible to move the entire equipment. It should, therefore, be possible to interest capital more easily in transportation. But I believe it will be difficult to interest it in the development of land and that transportation facilities will cease to carry as much of the burden in connection with suburban land development as they have heretofore.

WALTER JACKSON:—I confirm Mr. Manly's remarks. In developing a suburban community, if additional motor-bus service is delegated by the existing transportation company to another company which is known to have a reputation and will not remove its service, we may feel assured that that motor-bus route will continue to serve a particular group of people. If, on the other hand, motor-bus service is rendered by a new organization, which gives no assurance that its service will remain a fixture, the fact that the bus route can be changed without removing any rails is worth serious consideration.

HERBERT CHASE:—Mr. Jackson said that the cost per seat-mile for electric railways is about one-half that for motor-buses. Were the capital investment and the upkeep of the right-of-way considered in both cases? Is there any inherent reason why motor-bus transportation should be more expensive than railway transportation? There must be some basic reason for differences as great as Mr. Jackson mentions. So far as the vehicle itself and its cost of upkeep are concerned, I fail to see why Mr. Jackson's statement is correct.

On what grounds does Mr. Jackson base the conclusion that it is hopeless to try to beat the electric railway in cities by the use of motor-buses? They are competing successfully and giving satisfactory transportation in some cities in which the electric railway has, for one reason or another, been forced to discontinue.

MR. JACKSON:—That is a most important question. The comparative costs have two grounds. I refer to a paper read by A. R. Fearnley, of the Sheffield Corporation Tramways, of England, in which a really classic comparison was made of the cost of both kinds of service by a man who is known as a great advocate of the motor-bus. However, I am not satisfied with quoting English figures alone. According to American statistics a one-man safety car seating 32 to 35 people has so much overload capacity that it can carry 60 people easily and be run for 18 to 20 cents per mile, but a corresponding motor-bus cannot be run for that amount. When I speak of the 2 to 1 ratio, I refer only to the operating expense. However, with a service where the car intervals are only 10 min. and the vehicle seats 24 to 32 people, the investment for an electric railway would be so large as to make the cost of the street-car greater than that of the motor-bus. It is dependent upon the density of traffic. In a series of studies for the Boston Elevated Railway I recommended the adoption of the motor-bus for four or five localities where the company had track at that time. I pointed out that the cost of renewing and replacing that track was great and that, because the number of car-miles per year was not more than 50,000, new track and paving would not pay. On the other hand, I showed that if the company ran 500,000 car-miles per annum over the same track, the investment would be the same but the overhead charge so much less that motor-buses, at the existing costs, could not be considered.

The statement which Mr. Chase has made as to the replacement of trolley lines in some cities is true. I have said that some companies are trying to carry too many burdens of the past. They want a rate of return on a worthless valuation, and that necessitates a rate of fare that can be met by the motor-bus. That might be true of cities of 25,000 to 50,000 population, but we must consider each case individually.

TRACKLESS TROLLEY-CAR

L. C. MARBURG:—Who has had experience with the trackless trolley-car? I think it has been used to a limited degree for 15 to 20 years. It never has had a broad application, although recently I have read of a few additional instances where it has been installed.

A. J. SCAIFE:—One objection to the trackless trolley-car is that it is confined to a fixed route. One advantage of motor-bus transportation in large cities is that, if there should be something blocking the highway, the traffic is not held up but can seek other routes. It has been stated that it is not practicable in large cities to have street-cars run in the center of highways. Present laws stop all vehicles at the rear of a street-car while it is taking on or discharging passengers; thus in some instances from 10 to 30 vehicles are halted behind a car. With present-day congestion this is one of the greatest problems of mass transportation.

G. A. GREEN:—From my viewpoint, the trackless trolley-car lacks altogether the flexibility of the bus. It costs as much to build, its weight is as great and it possesses no advantages from the standpoint of acceleration or increased average speed. The cost of the necessary overhead structure ranges from $6,000 to $10,000 per mile; so that the cost of every route-mile operated is approximately equal to the cost of a completely equipped bus. Since the cost of the trackless trolley-car without the installation or maintenance of overhead structure or powerhouse equals that of the bus, the unit of investment cost is considerably in excess of that of the bus.

There are other serious disadvantages. Before trackless-trolley operation can be commenced, the overhead structure must be installed. If it becomes necessary to abandon a route, a large portion of this expenditure is lost. Likewise, extensions of routes cannot be effected promptly. Diversions on account of fires, parades, traffic blocks and the like are also impossible. Of course, a system of alternative routes is entirely out of the question.

The vehicles as a whole would be no cheaper to maintain. There is no reason to expect greater mileage from tires or decreased body maintenance. The electrical equipment is not more reliable than the conventional gasoline engine, clutch and transmission, and it does not seem likely that it will be cheaper to maintain. It must be remembered that the maintenance of motors, controllers and the like on steel rails is different from their

maintenance when vehicles travel over rough roadways. Certainly, the electrical equipment is not as well understood as the corresponding gasoline units. So, in the last analysis, about the only possible saving is in power. Here the question admittedly is open to argument but, in whichever way one regards the situation, the possibility of marked economy is not particularly alluring. If trackless-trolley operation is confined to points where power is extraordinarily cheap, the matter might well receive consideration; but, in my opinion, the disadvantages far outweigh the possible benefits.

It seems natural that the trackless trolley-car should receive attention from street-railway men, for they have been brought up to think in electrical terms. But, judging from such knowledge of these matters as I possess, I am of the opinion that one should never attempt to do anything electrically that can be done mechanically.

Chassis Lubrication

Cornelius T. Myers:—Mr. Green omitted upkeep expense in his recapitulation. The street-railway people are very much interested in the upkeep of these gasoline vehicles. The upkeep of those adapted more or less successfully to bus service has been pretty high. It is not safe to use a lower average rate of depreciation than 20 per cent per year.

In visiting service-stations and checking-up on repair costs, I find that a large portion of repairs is on the small parts about the chassis that never get lubrication. I have been working on chassis lubrication for about 10 years. I prepared statistics to show that operators of motor vehicles are justified in neglecting chassis lubrication. Considering 10,000,000 registered vehicles to be equipped with the devices now furnished by manufacturers for lubricating chassis parts, it would take a minimum of about 2 hr. per week per vehicle to give the chassis thorough lubrication. The minimum repair-charge for those vehicles would be $65,000,000 per year; the maximum, about $300,000,000; and a safe average probably somewhere between $100,000,000 and $150,000,000 per year. At 50 cents per hr. for 2 hr. per week per vehicle, $500,000,000 worth of someone's time is required. Hence, the public is absolutely justified in disregarding the lubrication of its chassis, many of which fail merely on account of the small chassis parts that are essential to the operation of the car. It is possible to lubricate those chassis. Their maintenance depends to a great extent upon lubrication at a minimum of 15 to 30 min. once in 2 months; that is, six times per year. With some 300 Class B trucks operating in one fleet in New York City, some remarkable records of repairs on chassis parts may be seen at the Army repair-service station. The cost has been exceedingly low. Certainly this is a field to be developed. If a minimum depreciation of 20 per cent per year in the chassis must be accepted, very few transportation companies will invest heavily in motor vehicles.

Mr. Green:—In my opinion Mr. Myers has placed undue emphasis on this question. The matter of proper lubrication is not a very difficult problem. It is largely a question of design. In our case we believe we have taken care of this matter satisfactorily. It is necessary to lubricate our vehicles but once in every 2000 miles of service, and the entire process requires about 15 min. The system is neither wasteful nor expensive. I think we can dismiss the question of lubrication as a controlling factor in the maintenance-costs of gasoline-propelled vehicles. This, of course, is on the assumption that the vehicle design is reasonably correct.

H. W. Slauson:—One subject, which possibly may seem outside the province of engineering but which will tend to make or break a motor-bus line, is the protection afforded by State legislation. Mr. Green probably would say that municipal competition is not serious, and we certainly hope it will not be. We see no reason why it should be serious, but there are instances in which a State has passed a well founded law in which bus rates are based on certain factors and these laws have been over-ridden by municipal and county legislation with the result that almost as many laws prevail in that State as there are counties. When a company is formed and money is invested in a bus line, there must be assurance that it will be protected from legislation as well as by it. It is unfair to an investor to face the competition offered by a municipal or county line which is not operated on the same basis so far as franchises, taxes and license fees are concerned. We are trying to overcome that by the uniformity of the vehicle law, but that is by no means perfect. It has failed in several instances. I believe this is one of the serious problems with which the motor-bus line is confronted.

Illumination

L. C. Porter:—I have been following with interest the development of motor-buses for the last 4 or 5 years, particularly in connection with the lighting, and have wondered at the lack of adequate illumination. Illumination has increased in trolley cars; in fact, in a number of cases, this has been required by public-utility commissions. I think the same requirement will apply to the motor-bus. I believe that the companies operating motor-buses would do well to forestall such legislation by supplying sufficient light. The original individually owned jitney bus was more or less unpopular, but as the motor-bus has developed and is now operated by competent companies on regular schedules, this opposition is gradually passing. Suitable lighting of buses would gain popular favor and be good advertising. A bus should not only be well lighted inside but carry illuminated signs visible from a distance to announce its route. It should also have adequate step lighting, and stop signals for the protection of those driving behind it.

Many buses have what are called dome lights. They are very inefficient, absorbing from 50 to 60 per cent of the light. With reflectors instead of domes, most of that light could be made useful. Why do the builders allow such low head-room in motor buses? If the roof were 1 ft. higher, the use of highly efficient reflectors would be possible and satisfactory lighting could be had with a small amount of electrical energy.

A Member:—I was one of the contenders for a motor-bus franchise in Chicago, but did not go through with the proposition because there are five different municipal bodies to contend with. After I had talked to these representatives, my opinion was that whoever secured that franchise would fail. The pioneer, who secured the franchise, failed within 3 years. I think the service has been continued for about 3 years since that time and that the company has recently been purchased by another organization for about 25 cents on the dollar. I feel that now with proper men, it would make a good investment.

On the South Side of Chicago, on Michigan Avenue, there are a number of viaducts. The municipal commission controlling Michigan Avenue insists that a certain route be covered to obtain a franchise. Can a six-wheel motor-vehicle of the trailer type be built with low enough head-room to afford the requisite passenger-carrying ca-

pacity? To secure that franchise virtually a new type of vehicle must be developed to make the route pay.

MR. GREEN:—From a motor-bus standpoint, a low center of gravity is highly desirable because the possibility of overturning is decreased and the stepless feature is of value in avoiding accidents in boarding and alighting. There is also an opportunity to shorten the time required by passengers to enter and leave. Assuming a correct suspension system, the riding properties are better. Decreased height lessens the liability to injury of persons seated on the upper deck. A low-hung vehicle also has a more graceful appearance. Such vehicles look safer and one must not lose sight of this from the rider's standpoint.

Regarding the question of lighting, I agree in general that the present situation leaves something to be desired. There is a tendency to demand better illumination. Much difficulty has been experienced in the past in obtaining generators of satisfactory capacity, but, as a result of experiments, our research department has made progress in this direction and within the coming year we expect to effect marked improvements. The problem of properly illuminating a bus is by no means simple. Each of our vehicles, completely equipped in accordance with our latest standards, requires 224 cp. This is beyond the capacity of the average generator in use on passenger cars.

MR. CHASE:—Is there an inherent difference in the cost of operation of a motor-bus and an electric street car, aside from the matter of size? It is evident that if a vehicle can carry twice as many passengers with the same crew, the operating expense will be reduced, but is there a difference in upkeep expense and with which is it the greater? Is it considered desirable by the majority of engineers to have a special type of chassis for buses, or is the conventional truck chassis, with certain minor body modifications, applicable?

H. C. MCBRAIR:—In one locality where a large number of motor-buses run from other communities into the city, an excessive tax has been levied on the motor-buses. The railroads are putting on motor cars to regain the trade that has been going to the motor-buses.

SPECIAL FEATURES OF BUS CHASSIS DESIGN

R. E. FIELDER:—As to whether it is necessary to have for motor-buses a different chassis from that used for truck work, I would say that human freight needs a definite amount of space and a bus body must be at least 7 ft. wide, to provide comfort for the passengers. The seats usually are along the side walls or arranged transversely; therefore, the supporting wheels and springs are placed on as wide a base as possible. In most cases it is desirable to have the width about 14 in. greater than that of the truck; otherwise the vehicle is unstable. Few steps should be used, particularly in cities where many people get on or off at one time and cannot see where to place their feet. Therefore, the vehicle floor should be as low as possible. The frame of a standard truck is about 36 to 40 in. above the ground. Since a passenger can step up only about 12 in., four steps are required for entrance to the body and they will stretch across the floor so that no platform space is left. These are some of the many features that require a bus to be of special design, and that make a truck chassis totally unsuitable for safety and fast passenger-bus operation.

OPERATING COSTS

MR. MARBURG:—As to any inherent difference between the cost of operating a motor-bus and that of a street-car, I think Mr. Jackson gave the answer when he said it is a question of traffic density. The cost of operating an electric railway consists of maintenance charges, the cost of the personnel necessary to operate the cars, the cost of electric current and fixed charges such as interest and depreciation. In the case of the motor-bus the place of electric current is taken by gasoline or oil. Some charges are proportional to the mileage; others are entirely or practically independent of it.

Taking first the variable charges, there hardly can be a doubt that cost of upkeep is considerably higher with the motor-bus than with the street-car; and, furthermore, it seems to me that the cost of gasoline and lubrication is also much higher than the cost of electric current, all charges being, of course, based on a seat-mile. On the other hand, fixed charges, consisting largely of interest and depreciation, are much smaller with the motor-bus than with the electric street-car, because the investment per seat-mile is much less with the former.

Since fixed charges must be distributed over the number of seat-miles operated, it is evident that the system with low fixed charges, that of the motor-bus, will have a total cost per seat-mile which is more or less constant; but with the system having high fixed charges, that of the street-car, the total cost per seat-mile decreases rapidly with increasing seat-mileage. The result is that the motor-bus operates more economically with low traffic density, and with high traffic density the street-car is cheaper.

MR. GREEN:—To attempt to analyze the difference in cost of operation between street-cars and motor-buses is a rather large undertaking. The street-car has one feature the bus has not; people can be packed into it. I am, of course, referring to "standees." It has an almost unlimited capacity for overloading; so, when discussing costs, one must consider whether reference is made to cost per passenger carried, or cost per seat-mile or cost per car-mile. Taking the cost per car-mile, in units of about the same seating capacity, the difference, I think, would be in favor of the motor-bus.

MR. MARBURG:—Are we not making an assumption that does not apply? The electric car always has a much greater seating capacity than the motor-bus.

MR. GREEN:—I think that statement is not altogether correct. Our single-deck bus seats 29 passengers; the double-deck bus, 51 passengers; the average New York street-car seats only 40 passengers.

CHAIRMAN B. B. BACHMAN:—Col. F. W. Perry, representing the Hon. Grover A. Whalen, commissioner of plant and structures of the City of New York, is present and we shall be glad to have him address the meeting.

MUNICIPAL OPERATION IN NEW YORK CITY

F. W. PERRY:—The Commissioner delegated me to address this technical meeting in his stead. Mr. Whalen is operating trolley lines, bus lines and trackless-trolley lines in the name of the municipality. The experience covers only about 2 years. The necessity for operation by the municipality has been due to a public emergency existing in New York City in the nature of no transportation if not public transportation. The trolley lines were wholly abandoned and the cars disposed of. The city took over the lines and put on the cars, took the people to church on Sundays, took the children to school and gave the business people a chance to reach the ferries and get to business. The bus lines took the place, by and large, of railways that had abandoned operation. These trackless-trolley lines are operating on Staten Island and extend existing transportation lines into the territories that need transportation. They are not for real-estate

development, but to fill the existing demand for riding. They extend existing transportation lines that could not be extended by the operating companies in view of their financial condition.

The whole question is complicated and cannot be disposed of easily. I think that no competition is involved in any of this transportation. During a serious strike in Brooklyn, when the railway companies were unable to operate, the Commissioner ran upward of 1000 buses over the routes and continued running them until the companies were able to pick up the load; but, in the meanwhile, the service was enjoined and had to be shut-down.

Mr. Jackson has a very well anchored idea that the buses ought to be operated by the existing utility companies. There are many who think that way. Keeping in mind what the utility companies are carrying in the way of financial loads, there is nothing special in what has been said, other than what Mr. Green mentioned a moment ago, about the comparative cost of trolleys and buses. It seems to me that a very much heavier load is being carried in the case of the trolley-car passenger-mile than in that of the bus. This largely increases the cost of electricity. While the platform cost might not differ, when the present-day cost of electricity per kilowatt-hour is taken into account, there is a difference which is against the trolley, and particularly the trolley of the large Pullman type.

N. G. SHIDLE:—Mr. Perry mentioned one point in Mr. Jackson's paper that perhaps is misinterpreted. I do not pretend to speak for Mr. Jackson on the matter of advocating that buses probably will be operated by the utility companies that are in business at present, but I believe that the difficulty is as follows: As I understand it, truck builders who perhaps believe that there is a wide motor-bus market are not certain just where that market is. We can talk as we will about the great future of the motor-bus, but when a company turns its plant over to the building of special motor-buses, it must know where the output can be sold.

It is interesting to study the development of the motor-bus in the places where it already has been developed, in order to learn how it can be developed in other localities. Mr. Jackson has made a detailed study of conditions in England. He found, I believe, that bus lines there have, to a large extent, been developed by transportation companies already in the business that had seen the possibilities of tying the motor-bus to their present transportation lines to make a better transportation system. The public-utility companies in England have started bus lines or added bus lines to their present equipment on that basis; and, to some extent, perhaps that is the basis of the idea that the utility companies at present in the United States are likely to be the ones which will promote the bus lines.

Mr. Perry said that he believed this matter of buses and trolleys is not one of competition, and I most heartily agree with him. Fundamentally, the motor-bus is a transportation unit. Just as it was found that the only way the motor truck could make progress was by selling it as a transportation unit and not as a motor truck, the motor-bus will take its place as a unit in our transportation system. If it is found to be a good transportation unit after proper tests and trials, it is bound to take its place. The matters of State legislation and municipal competition with privately owned bus lines, as has been mentioned, undoubtedly constitute most difficult problems confronting the development of the motor-bus; but, if in each particular case the bus proves that it can make money, it will be used, regardless of the attitude of the electric-railway people. When the public finds that the motor-bus can help it out, buses will be used. There are some cases in which the motor-bus is the most economical transportation unit, certain other cases in which the electric railway is the logical transportation unit and many intermediate cases. It will require a long time in the intermediate cases to determine which is the better. It will depend to a large extent upon the capital already invested in the enterprises and the returns being made on that money, rather than upon furnishing the most efficient transportation to the community. Eventually, efficient transportation will be furnished, whatever it shall be; the result will depend partly upon the possibility of developing the motor-bus mechanically with due regard for the comfort of passengers. The remark referring to the truck that carries freight and the bus that carries human beings as freight tells the whole story; there is a necessity for something different.

COLONEL PERRY:—I thought I made it clear that I was not indulging in optimism about any particular kind of transportation. I was merely stating what the City of New York is doing; it is running these buses, but not in competition. There is ruinous competition going on in other cities. I want to distinguish what is being done in New York City from what is being done elsewhere. We do not run jitneys to put trolley cars out of business. I hope everyone understands that. Anyone who has made a study of the question knows that if trolleys are too expensive, we would like to sell them and run buses.

MR. SCAIFE:—When the operating cost of the motor-bus is compared with that of the street-car, one should take into consideration the trackage cost, which, I understand, is from $40,000 to $60,000 per mile of double track, and that the safety car costs about $8,000 or $10,000. With the motor-bus, the investment is entirely in the rolling stock. So far as the operating cost of the street-car per seat mile is concerned, that cost is divided over the entire period because the overhead charges are fixed. With the motor-bus, that is not the case; trackage and trolley overhead does not have to be charged against the seat cost per mile.

With reference to the use of the motor truck as a motor-bus, I believe we are beginning to realize that it is impossible to use a motor-bus that is built on a motor-truck chassis. I believe the bus must compete with the comfort of the average street-car, with reference to lighting and riding. The jitney undoubtedly has injured the motor-bus in its pioneer work, where any kind of a body was put on a truck chassis and the job was called a bus; rough-riding, uncomfortable and poorly lighted. I believe that has done more to retard bus development than anything else. However, as Mr. Green stated, electric companies are making generators that will take care of bus lighting, and the bus builders are designing chassis that will have easy riding qualities.

With reference to legislation, years ago in New Zealand, I believe a franchise to operate a steam railroad was given provided the company would erect a fence along the entire right-of-way so that people would not become panic-stricken at seeing a train travel at the rapid rate of 20 m.p.h. As far as legislation is concerned, everything new must go through such a period of opposition.

J. E. HALE:—To the rider, the most important elements of adequate transportation are the maintenance of schedules, comfort in riding, safety and speed. There is a general reluctance to have street-cars in residential sections. I mention these things because the cost is not the only consideration. Factory workers, who must

watch their pennies, select the most economical means of transportation. Who can indicate whether a good, efficiently operated, motor-bus system can transport people from point to point more quickly than can a street-car system? That is a very important point. In many instances people take a jitney in preference to a street-car because the jitney will make the trip in less time.

Nearly all the electric street-car companies serve the public with electricity and power, the transportation department in many cases being financed to some extent in this way. If street-cars were removed and trolley transportation abandoned, what would the effect be on the light and power service the companies give the communities?

HARMONY IN CAR UPHOLSTERY

BY R. S. QUAINTANCE

TAKING the artist's viewpoint, the author discourses on the subjects of color, color harmony and the psychological effects of color, as a prelude to a discussion of how the decoration of car interiors can be made most effective, this being necessary because of the elusive quality of good taste, a quality of appraisal rather than one of creation, and because esthetic taste depends upon the degree of mental development of the individual, although possessed in some degree by all.

The primary, secondary and complementary colors are defined and their mode of selection is described preliminarily to a consideration of color values and the selection of the most effective color-schemes, application of these principles being made thereafter to the decoration of car interiors, inclusive of comments on the most suitable fabrics and patterns. The author believes that color will be considered eventually in the automotive industry as being on an equal plane with lines and form. [Printed in the February, 1922, issue of THE JOURNAL.]

THE DISCUSSION

A. J. NEERKEN:—What colors have the greatest popular appeal? In the quantity production of a medium-priced car, where only one color or one type of upholstery can be used, what color would please the greatest number of people, and what the second greatest number?

R. S. QUAINTANCE:—It has not been determined what color is the most popular. We must be guided by the color selected by the car builders. We have no means of determining which color would actually be the most popular with the purchasers of cars. Blue, probably, is the most popular; gray comes next.

CHAIRMAN E. G. BUDD:—Which color would show the more correct taste?

MR. QUAINTANCE:—Both show correct taste. One would not say, for instance, that red is a correct color for the interior of a car. Both blue and gray are suitable; blue, however, is a cold color. Gray is neutral; it is neither cold nor warm. Blue is extremely popular. I think brown is the most desirable color for a closed car, because it is one of the warmer colors.

MR. NEERKEN:—Is it not necessary to choose a more or less neutral color so that it will harmonize with what people wear? If one chooses a bright distinctive color, it will not appeal to many people.

MR. QUAINTANCE:—On the other hand, a neutral color is ultra-conservative. A blue color need not be disagreeable; in fact, it can be made agreeable very readily. We would not use a pronounced blue, which might affect certain personalities. Blue is the coldest of all colors, and some types of blue actually might be depressing. By mixing or toning the blue down to some of the warmer colors, one could effect an almost neutral color harmony by using blue as the dominant color; it would not, however, be an intense blue.

J. E. SCHIPPER:—Is there anything about the colors themselves that makes them more or less susceptible to fading, thereby causing unevenness in the appearance of the upholstery?

MR. QUAINTANCE:—Yes, some colors will fade. Blue will fade first. Tones of red come next. It is also very difficult to prevent green from fading. However, manufacturers today are able to produce fabrics that will not fade readily, but in time the sun will affect all shades of blue.

A MEMBER:—Is it not true that there is a manifest tendency toward the red-grays in upholstery? Gray is suitable as a background for harmonizing any body color. Red-gray is more permanent and is warmer.

MR. QUAINTANCE:—That is true; it will produce a very harmonious, interesting color scheme.

A MEMBER:—Is there not some rule to determine what the color of upholstery should be to harmonize with the body color outside?

MR. QUAINTANCE:—There is no fixed rule; a wide latitude is provided for body-color paint. Colors must be selected with a knowledge of their relationship to one another. If the dominant hue on the interior is on one side of the color circle, one is privileged to go 180 deg. on that circle to obtain a suitable color for the outside; or, one may use a color distinctly related to the interior.

A MEMBER:—Suppose one used a maroon color for the outside of the body, would it be safe as a rule to use gray for the inside? That would harmonize with the outside maroon color.

MR. QUAINTANCE:—That would be a safe rule, but one does not wish to limit the choice to that; there are other equally safe combinations.

A MEMBER:—Is not the tendency of the times more toward that type of decorating than toward any other?

MR. QUAINTANCE:—I have not noticed that there has been any fixed rule in regard to applying any particular color in preference to any other. My observation has been that there has been no great departure from a related color scheme between the exterior and the interior.

A MEMBER:—Is not the tendency always toward a gray, if possible, for the interior?

MR. QUAINTANCE:—Yes.

CHAIRMAN BUDD:—In regard to the point of emphasizing the interior by spots of color, how can that be done on the outside of the car?

MR. QUAINTANCE:—Most of the car exteriors are so conservative that I have not been concerned about them, but I think it would not be necessary to put spots of color on the exterior if the cars were painted some color other than black. The interior primarily exerts an influence upon the passenger and that influence may be either agreeable or disagreeable. In the case of conservative harmony schemes, we can eliminate much of the depress-

ing sameness that is to be sensed in some interiors by an occasional spot of complementary color.

CHAIRMAN BUDD:—Most cars start with black on the hood and fenders, and usually are black on the roof.

MR. QUAINTANCE:—Black is neutral and can be used with any color in the circle; the same thing is true of gray and white.

CHAIRMAN BUDD:—Would no body color that we might make clash with black?

MR. QUAINTANCE:—No, although one could use a strong contrast. White would be the strongest contrast, but white is not a color. Yellow would be the strongest contrast, but that would not be discordant.

G. W. KERR:—I think that the last paragraph of Mr. Quaintance's remarks about body color is hardly justified by the facts. From the beginning, automobiles have been painted practically all colors of the rainbow. The all-black car is the exception rather than the rule. Many production cars are painted blue and others are painted green. Other colors than these are and have been used continuously since the automobile came into use. I think he laid hardly enough stress upon the subject of making the upholstery match or harmonize with the paint that is selected for the outside of the car. There is a harmony of contrast and a harmony of matching. With a color like yellow, for instance, the best contrast would be blue. A color frequently is selected for the body of a car that has a complementary or best contrast color not in itself pretty or one that appeals to the eye. In a case like that, it is necessary to use some other part of the color circle that will provide harmony. Maroon is a color difficult to harmonize by contrast because its complementary lies in the greenish-yellows, which are very poor colors and not handsome; but it always is possible to harmonize with maroon by matching and using a material corresponding with it. Another important resource in harmonizing upholstery with exterior finishing is the use of the neutrals, white, black, gray, silver and gold. Silver and gold harmonize with anything; white and black are suitable for use with any color, and so are the grays usually. If harmony cannot be attained with a certain color, we may resort to a neutral and obtain a contrast of a slightly lower tone than is found by using the complementary.

G. J. MERCER:—Selecting fabrics from small samples of goods will at times be disappointing. That is to say, when the fabric is used to trim the interior of the car and a larger quantity is used, the appearance may be different from the impression one gets from the sample. Do any particular shades have that tendency more than others?

MR. QUAINTANCE:—Not that I have observed. I believe that possibility applies to all colors. On the other hand, a large piece of cloth will also be deceptive when draped as cloths generally are. The color that is synonymous to light will catch and reflect the light and produce on a large piece of cloth a sheen, a glow, which will be equally as deceptive as a small piece of cloth would be. It is necessary to use one's imagination largely when selecting fabrics. Trimming a sample job and then determining its value is a good plan that has been tried. That is one of the safest ways, although an expensive one, to determine whether a particular cloth will meet the needs of a particular company.

MR. MERCER:—One fabric manufacturer used to show all the laces, cloths and silks draped under conditions similar to those of the interior of a car. Is that method still followed?

MR. QUAINTANCE:—A number of car-fabric manufacturers will furnish, with the samples of the fabric, samples also of the laces, the seaming to be used, curtain silk, carpets, curtain cord and the like, but I do not believe they enable one to determine whether the cloth will be satisfactory after it is installed. They merely aid in visualizing in a measure the color harmony that is possible in the interior through the use of the different trimming accessories. Do you mean that some fabric manufacturers used to trim a model car?

MR. MERCER:—No, the materials were draped in a manner to give a good impression of the back and side, for instance.

MR. QUAINTANCE:—I have never seen that done. It is customary with my firm to show cloth draped over an arm. That will demonstrate how the cloth will tuck in on the plaits and corners, and enable one to get a conception of how it would look on the sides. I know of no one who provides an atmosphere that would enable a customer to judge correctly how the materials would look in tht interior of the car itself.

W. C. KARNS:—My experience has been that what appeals to one person does not appeal to another. Would you not deem it advisable to change the various fabrics to comply with the varieties of taste?

MR. QUAINTANCE:—Yes. One of the serious mistakes of merchandising lies in offering only one example of car interior to customers. In my opinion they should have a choice, because certain colors produce definite effects on certain personalities. For instance, the red colors are warm and the blue colors are cold. The red is stimulating and the blue is depressing. To a nervous, volatile type of person, a color on the warm side of the circle might be sufficiently stimulating to be really exciting. That person probably would feel at home and be more comfortable with a color scheme based on the warm side of the circle because of his nervous temperament. On the other hand, the phlegmatic type would probably prefer the blue side. But that side, if he only knew it, would produce a feeling of depression and be too soothing and quieting, although it might appeal to him more than the warm side. There is a definite personal feeling to be secured from colors and whether people are conscious of it or not, they respond to their influence. Almost everyone has sensed color, even though he has not been able to define his feeling of comfort or happiness in the presence of certain hues. Comfort or happiness or depression is the result of the proper or improper use of colors. A choice should be provided by the car builder because of this wide variety of personalities among customers.

CHARLES F. HOPEWELL:—The paper can be divided into two distinct sections, one relating to car-upholstery color-harmony and the other to upholstery materials. I agree fully with Mr. Quaintance's plea for better color-harmony. He states the fact that the less obvious the color element is, the more quality the different tones possess. He also states that tints and shades rather than pure colors should be used. One of the most effective means of obtaining this tone of color is the use of cut and uncut-pile mohair-plush. We are all familiar with this type of plush, as it is a standard in Pullman cars.

I disagree in regard to his conclusions as to woolen fabric being the best fabric to use for closed-car upholstery. However, Mr. Quaintance does state that mohairs woven from the hair of the angora goat are extremely serviceable, and he further states that velours give a rich, luxurious effect. The United States Department of Agriculture Bulletin states that mohair has a luster peculiarly its own. Therefore, does it not necessarily follow

that mohair plush is extremely serviceable and most luxurious? I think this alone should disprove Mr. Quaintance's contention for the use of various kinds of weaves of woolen cloths in preference to mohair plush.

Closed-car upholstery should possess durability, sanitary qualities and good appearance during the life of the car. In regard to the durability and sanitary qualities of mohair plush, let me recall to you the paper presented by Charles B. Dudley, who was for many years head chemist of the Pennsylvania Railroad, before the American Public Health Association. His conclusion, after exhaustive tests covering a period of more than a year on a variety of fabrics, was that from the standpoint of health a mohair-pile fabric is the only one that should be used. As regards durability, we know that they are used universally in railroad cars, as no other fabric has been found equal to mohair plush for wear.

The last few years have seen the development of mohair tapestry plush now being used on furniture that can be seen in the newer hotel lobbies. It has been adopted for both its appearance and wearing qualities. It is now available for car upholstery. Many fabrics possess, at the time of the sale of the car, all the requisites of appearance; yet none surpasses in appearance or possesses the wearing qualities of mohair plush with its pile fabrics.

We all know that it is impossible to keep oil, grease, dirt or tar-laden dust out of a car. Mr. Quaintance recommends the protection of the car upholstery by slip-covers, during the period in which the windows of the car are open. Such covers are absolutely necessary with woolen fabrics, but are not necessary with mohair plush. After a woolen, or non-pile, fabric has been spotted with oil or tar and dust, if we seek to remove the spot, say with gasoline, we do a poor job at the best, and the location of the spot becomes manifest as soon as the upholstery is again exposed to dust. This is not true with mohair-pile plush, as any spot that may exist is on the woven back of the plush and the pile face hides it entirely.

I do not say that there is no field for a woolen fabric for closed-car upholstery, for in many cases, where price governs quality, such a field exists. Where quality, durability and continuous good appearance govern, mohair plush is supreme for use as closed-car upholstery.

MANIFOLD VAPORIZATION AND EXHAUST-GAS TEMPERATURES

BY O. C. BERRY AND C. S. KEGERREIS

STATING that present internal-combustion engine fuel is too low in volatility for economical use and that this is the cause of engine-maintenance troubles, the authors believe that, since it is not possible to obtain the more volatile grades in sufficient quanitity, the only hope of remedying this condition is to learn how to use the heavy fuel, and that the most promising method of doing this lies in the effective use of heat.

As the experimental data regarding the best temperature at which to maintain the metal in a hot-spot manifold and the range of temperatures available in the exhaust gases, are meager, the authors experimented in the Purdue University laboratory to secure additional data. They present a summary of the results. They feel that the exhaust-gas temperatures are high enough so that properly designed manifolds, together with thermostatically controlled carbureter temperatures, should make possible the satisfactory carburetion of fuels considerably heavier than the present "power" gasoline, without seriously limiting the power, efficiency or flexibility of passenger-car engines or causing any engine-maintenance troubles. [Printed in the March, 1922, issue of THE JOURNAL.]

THE DISCUSSION

P. S. TICE:—It lately has been impressed upon the minds of automotive engineers that at least a close approximation to complete vaporization in the intake is essential. Not only does it increase the utilization of fuel but it is utterly impossible without it to use fuel-metering characteristics in the carbureter that result in maximum utilization. When considering the use of exhaust heat to produce vaporization, it must be noted that as combustion of the fuel in the cylinder become more complete less heat is available in the exhaust. However, the ratio between the heat available and the heat needed for vaporization always increases as utilization is increased.

A study of the curves presented by Messrs. Berry and Kegerreis brings out the fact that all the data presented were obtained under conditions of comparatively small utilization. For instance, consider their curve of exhaust temperature versus mixture-ratio. With the greatest possible utilization, this curve is not only lower on the temperature scale but it has a different shape and a different general slope. What complete vaporization really does is to give to the effective mixture in the cylinders proportions equal to those of the overall or metered mixture. Unvaporized fuel does not burn; hence, the effective mixture is poorer in fuel than the overall or metered mixture by the relative amount of liquid in the cylinders at ignition. There are, of course, other aggravating circumstances. Chief of these is the one that admission of liquid fuel to the cylinders cannot result only in non-homogeneous mixtures.

The brake thermal efficiency of just over 13 per cent at one-half load, as in the curve in Fig. 6 of the paper, presupposes an overall or gross mixture-ratio of between 9 and 10. While it is probable that the brake efficiency here shown was not the maxmium that could have been obtained under the conditions established by the authors at one-half load, it is equally probable that it could not have been much higher. In this connection it is interesting to note that, with complete vaporization and therefore equal distribution and high utilization, such an engine at the same speed and relative load shows a brake thermal efficiency of 22 per cent with a mixture-ratio of between 15 and 16. Experimental observation shows that, in general, with conventional service methods of vaporization, an overall ratio of between 9 and 11 is required to secure an effective ratio of from 13 to 16.

The authors' results on vaporization from a hot plate are interesting, even if they are inconclusive. They at least point out the fact that it is easily possible to secure data from which one can design to secure fuel vaporization.

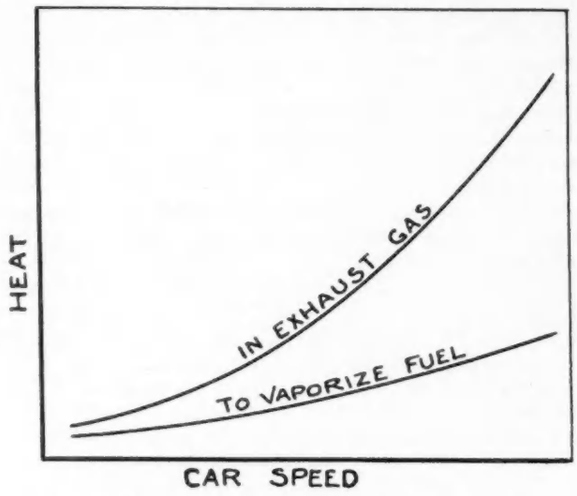

FIG. 4—CURVES SHOWING THE GENERAL RELATION BETWEEN THE HEAT REQUIRED TO VAPORIZE FUEL AND THAT PRESENT IN THE EXHAUST GAS AT DIFFERENT CAR SPEEDS

In any car, it is possible to plot with great definiteness the two extreme sets of conditions surrounding vaporization of fuel by exhaust as shown in Fig. 4. I made such plottings some years ago for several cars that were available for experimental work. I was forced to conclude, with Messrs. Berry and Kegerreis, that the only doubtful combination of conditions might be that existing at the smallest loads at the lowest speeds. But, even

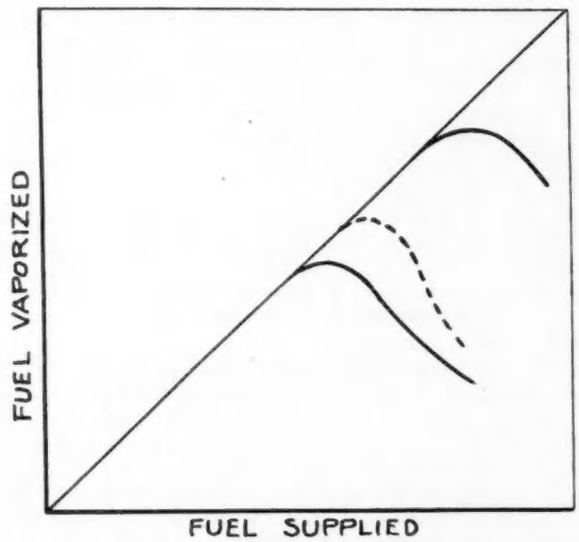

FIG. 5—CURVES OF THE CAPACITY FOR VAPORIZING FUEL

there, considering that the heat is available only at comparatively low temperatures, its quantity and the temperatures at which it is available are sufficient for our needs if advantage is taken of the conditions that exist in a throttled engine. The only thing that the designer is forced to create for himself in such a case is a reasonably efficient heat-transfer from the exhaust gas to the fuel liquid.

My early work directed toward the attainment of this reasonably efficient heat-interchange showed definitely that a simple flow of liquid, either in streams or in a thin film over a heated surface, did not result in a usefully high vaporizing capacity expressed in terms of weight of fuel evaporated from a unit surface in a unit time. The authors' plots of time versus temperature of a hot plate show in some measure why this is so. Furthermore, it soon was found that the effective capacity of a unit surface depends very importantly upon the manner in which the liquid is brought to the surface; this, in general, was independent of the temperature of the surface.

Briefly, the highest vaporizing capacity of a unit surface results from finely spraying the fuel upon a hot surface at such a rate as just to fall short of wetting the surface. When the fuel is so applied, the vaporizing capacity of a unit surface having a given amount of

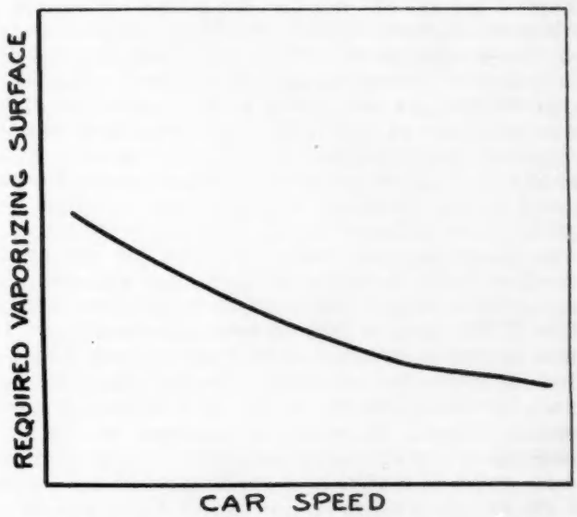

FIG. 6—THE RELATION BETWEEN THE REQUIRED AMOUNT OF SURFACE TO VAPORIZE THE FUEL AND THE CAR SPEED

available heat is an inverse function of the size of the liquid particles or globules. With an intake embodying this idea and arranged so that the vaporized fuel can be quantitatively determined, plottings of capacity assume the general form shown in Fig. 5. Each curve represents a constantly available quantity of heat at a constant temperature. As soon as the surface shows wetting, the ratio Q/V starts to fall with great rapidity. The dotted curve shows the general effect of increasing the division

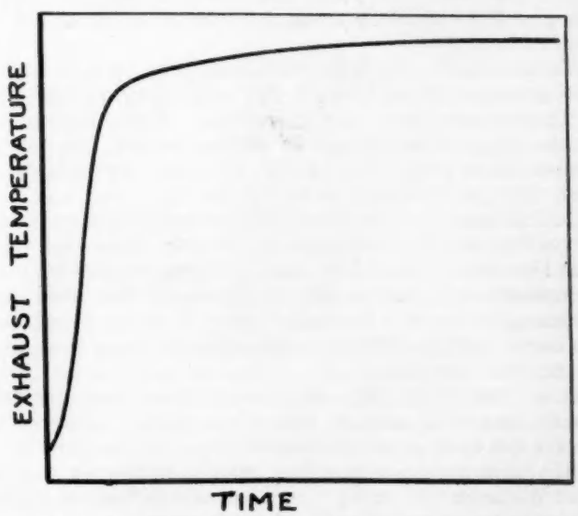

FIG. 7—HOW THE EXHAUST TEMPERATURE INCREASES AS THE TIME OF RUNNING INCREASES

of the liquid before its contact with the heated surface.

Not only does this spraying method greatly increase the capacity of a unit surface under given heat-supply conditions, but it makes possible the uniform loading of whatever surface is employed. Most important of all, I believe, the spheroidal condition is never found at any temperature encountered in such a system. From the spacings of the curves in Fig. 5, it is seen that an increased heat application under car-operating conditions results in an increase in capacity. This increase is substantially one of direct proportionality so long as wetting of the surface is avoided.

A plotting of the required vaporizing surface versus car speed, for any car, assumes the form shown in Fig. 6. Such a result shows that the end of the range of operation that determines the extent of surface for vaporization is that of the small load and low-speed. Having designed for this set of conditions, the remainder of the range takes care of itself. If it were physically possible to apply all the fuel in the form of a fine spray to a surface of 2 sq. in., this surface would be adequate to secure dryness in the intake of a Ford engine, provided heat could be made available to it at a sufficient rate.

The practically controlling factor in the use of the method of finely spraying the fuel upon a heated surface, is not the extent of the surface per se, but the application of the spray to that surface. That is to say, the heated surface must be so located and of such a size as to include all the fuel particles. The fact that the liquid is finely divided presupposes that it is occupying a considerable volume. To return to the case of the Ford engine, the extent of vaporizing surface to give complete dryness under all conditions is represented by the surface and one end of a cylinder 2 in. in diameter and ¾ in. long, or a total area of 8 sq. in. Further practical service considerations make it desirable to exceed by from two to three times the extent of surface actually needed to secure dryness under normal operation.

Let us consider the case where a man goes to his unheated garage, starts his car, drives off at about 20 m.p.h. and continues to run at that rate until the temperatures throughout the engine shall have come into equilibrium. The first temperature to do so will be that of the exhaust; but one must run for some 20 min. or more before that temperature becomes stable, as indicated in Fig. 7. This means that, for several minutes after starting, liquid fuel will pass into the cylinders and cause more or less trouble in addition to impoverishing the effective mixture.

To practically eliminate this condition, it is my practice to make the extent of the vaporizing surface two to three times that actually needed. This enlargement of the vaporizing surface in no way alters the normal temperatures of the charge, but it does enormously accelerate their rise to normal values.

L. C. MARBURG:—In these curves showing temperatures at varying speeds, how was the variable speed obtained? Was the spark changed or was the speed secured entirely by opening and closing the throttle? If the latter was the case, where was the spark set? It seems to me that the curve must be different with an early spark than with a late one.

E. D. THURSTON, JR.:—How were the exhaust temperatures measured and at just what point? I know of some experiments which showed that, in the movement of the thermocouple or other means employed, a very short distance will make a considerable difference in the temperatures found for the exhaust gases.

R. E. WILSON:—How did the temperature at which this spheroidal condition began compare with the end-point of the fuel? An interesting fact in connection with the work of these authors is that the composition of the mixture boiling at a constant rate on the hot-spot that is described was presumably identical with what we have prepared and termed the "equilibrium solution" in determining the "condensation temperatures" of fuels. It is also interesting to note that the temperature of the liquid when it takes on the spheroidal condition must actually be *lower* than that of the more rapidly vaporizing liquid when the temperature of the metal is lower. In other words, as we go for a considerable distance above the point where the spheroidal condition is approached, the *higher* the temperature of the metal is, the *lower* the temperature of the liquid will be, since the rate of vaporization of the liquid is a good measure of its mean effective temperature. This undoubtedly accounts for the fact that less decomposition was observed when the fuel was in the spheroidal condition than when the rate of vaporization was at a maximum just before the spheroidal condition was approached.

O. C. BERRY:—Mr. Tice has developed a method of vaporizing liquid fuel by spraying it on to a heated surface. As he points out, the amount of fuel that can be vaporized per square inch of surface is greater under these conditions than when the liquid flows onto the heated surface. We are therefore pleased to get his figures showing the rates of vaporization that can be obtained in this way. The fact nevertheless remains that the majority of heated manifolds makes use of the other method, and the figures presented in this paper are directly applicable to these cases.

We do not wish either Mr. Tice's remarks or our own to be interpreted as indicating that the critical condition for a manifold, or the one where failure is most likely to occur, is when operating at low speed and low load. This is more critical than when higher speeds or higher loads are maintained steadily, but the real danger point is when a heavy load is thrown upon the engine immediately following an extended period of slow-speed low-power operation.

In reply to Mr. Marburg, the various speeds were obtained by changing the load on the dynamometer. The throttle was wide-open in all cases for the full-load tests. The spark setting was a compromise and was kept constant at all speeds at the same load during a variable-speed series.

In reply to Mr. Thurston, the location of the thermocouple was near exhaust-valve No. 5. The thermocouple was allowed to project into the intake-manifold, into the gases themselves. Considerable care was taken to insulate the wire from the metal by asbestos, to prevent the flow of heat from the wires to the exhaust-manifold wall. One can well understand that the wires would have considerable heat-conducting capacity and, if they were brought into intimate contact with the colder metals of the exhaust-manifold, the temperature recorded would be much too low. It is true that the temperature will vary markedly with the location of the thermocouple. It is our effort here not to get exact figures that would be universally applicable, but simply to get a general idea of the magnitude of the forces with which we have to deal.

As to Professor Wilson's question of the comparison of the spheroidal state and the end-point, unfortunately I had not thought of it in that way. One can take from the curves the temperature at which the spheroidal state begins, and we have figures showing the distillation curves of the fuel in 10-per cent increments. The end-point is given in each case.

AIR-COOLED ENGINE DEVELOPMENT

BY CHARLES L. LAWRANCE

THE development of air-cooled engines for aircraft never made much progress until the war, when the British attempted to improve the performance of existing engines by a series of experiments leading eventually to the development of aluminum cylinders with steel liners and aluminum cylinder-heads with a steel cylinder screwed into the head. The advantages of these constructions and the disadvantages of other types are discussed. Results are reported of tests at McCook Field on a modern cylinder-design of this type showing good results, that lead to the belief that large air-cooled engines will be produced in the near future, equal in performance to water-cooled engines of the same power. It is claimed that, at present, there is nothing to choose in performance between water-cooled and air-cooled engines of about 25 hp. per cylinder, and that air-cooled engines of this size can be built successfully of the same compression-ratio and having the same fuel-consumption as high-compression water-cooled types.

An explanation is given of the reason for the advantages of aluminum cylinders and head constructions, and a chart is presented showing the temperature on the front and the rear of a large air-cooled cylinder of high output. The question of the cylinder-wall temperature of air-cooled and of water-cooled engines is discussed, and it is indicated that there is not much difference in temperature between the two types. The water-cooled engine is at a disadvantage on account of the number of heat transfers from one medium to another before the heat reaches the air. A statement is given of the reason air-cooled engines can perform satisfactorily with less cooling area than is required for water-cooled engines.

The subject of the resistance of airplanes having air-cooled and of those having water-cooled engines is discussed, attention being drawn to the fact that little is known of the best methods of installing and cowling these engines. Various suggestions are made for possible future development of cowling, to permit airplanes equipped with air-cooled engines to offer as little the resistance as those equipped with water-cooled engines. [Printed in the February, 1922, issue of THE JOURNAL.]

THE DISCUSSION

DONALD MACKENZIE: — I am almost convinced that the air-cooled engine is the future engine for aircraft. Has any measurement been made of the exhaust waste temperature? I believe these tests were made on the Hispano-Suiza engine. It appears that the power carried away in the cooling system was roughly one-half the power developed at the brake. Is it possible to estimate that?

CHARLES L. LAWRANCE:—As I understand the tests made at the Bureau of Standards, the amount of heat absorbed by the cooling system was about 12 to 15 per cent.

MR. MACKENZIE:—That refers to the total heat energy of the fuel, but the loss in brake horsepower would be about twice that amount.

MR. LAWRANCE:—I do not know that any such tests have been made.

S. D. HERON:—Regarding the heat dissipated by the external cooling surfaces of an aluminum-and-steel air-cooled overhead cylinder of the type mentioned in Mr. Lawrance's paper, this has been found to be approximately 60 per cent of the heat equivalent of the brake horsepower. In addition, heat approximating 40 per cent of that developed as brake horsepower is dissipated by the oil, crankcase conduction, radiation and the like.

H. M. CRANE:—Mr. Lawrance has given a very interesting history of air-cooled-engine development. The first thing we notice is that the air-cooled engine is not in its infancy. It is 25 to 30 years old. In fact, the early flying was almost invariably done with air-cooled engines. The trend of thought among airplane enthusiasts was invariably in that direction. It did not seem possible that the water-cooled engine could be used. One of the earliest practical water-cooled engines was the Antoinette, and that was rather an exception. We have Mr. Lawrance's word that the English Channel was first crossed with an Anzani radial engine, which is a fair example of an early type of radial engine. I think the air-cooled engine today can stand on its merits, and that it must admit that it had at least an equal start with the water-cooled engine. I think it started earlier. Even during the war, the water-cooled engine was on the defensive. One reason was that the designer of air-cooled engines visualized a radial engine about 36 in. in diameter, producing about 400 hp. and weighing about 500 lb. Some early advertising of the A.B.C. Dragonfly engine gave figures of that general order, although the engine had a greater diameter than I have stated. The actual results with radial air-cooled engines are different from what we have been promised in size and weight, especially when we reach the larger horsepowers.

What important features of construction do we find in the two engines at present? Most of Mr. Lawrance's drawings show radial air-cooled engines with single cylinders, which make them look reasonably simple. As a matter of fact, there is no comparison between the two types when it comes to rugged simplicity of construction. The 8 and 12-cylinder engines of modern water-cooled design are about as rugged when compared to air-cooled engines as the ordinary automobile engine is when compared to the water-cooled aviation engine. Mr. Lawrance has compared a very modern uptodate radial air-cooled engine, which I know is the result of years of painstaking effort on his part, with a water-cooled engine, which was produced in large quantities under the stress of war, and passed daily tests on the basis of the figures given today.

The only thing to do is to compare the latest uptodate air-cooled engine with the latest uptodate water-cooled engine under the same operating conditions, where the mean effective pressure of the water-cooled engine reaches 145 lb. per sq. in. This engine weighs 620 lb. and develops 400 hp. at 2000 r.p.m. on an extremely low gasoline consumption of less than 0.45 lb. per b.hp-hr. If we add the weight of radiator and proper amount of water, we still have a lighter weight than that of any air-cooled engine, which shows signs of running satisfactorily and develops like horsepower. When I say the water-cooled engine is a more rugged engine, I am willing to include with the water-cooled engine the necessary radiator and the attachments that go with it. I think no such radiator as was described for 220-hp. engines is necessary, because I am strongly of the impression that the 300-hp. engine was cooled with two radiators, each of which weighed less than 30 lb.

Granting that the radial air-cooled engine is justified

as an engine, there is nothing to indicate that it is a good airplane engine.

I went to France in 1915 with the responsibility of choosing an engine that could be built in the United States. The Salmson plant interested me most and was by far the best operated in Paris at the time. It had already produced a 250-hp. engine which was running excellently, but the plant was absolutely idle. There was not an order in hand except for a small number of engines for Russia. This was because the airplane designers recognized the fact at that time that the form of the engine was about the worst possible for mounting in an airplane, due to its high head-resistance. Later on, it is true, the stability of this engine, coupled with the services of a progressive and uptodate airplane designer, resulted in producing a very efficient machine in this plant, but the machines used in the war were of a distinctly slow-speed type, compared with those we have in mind today. We heard much during the war about speeds of 130 and 140 m.p.h., but they were not realized. The machines of 110 m.p.h. were the speediest of that time.

Not only is the form of the engine bad from the aerodynamic viewpoint, but it is bad for the mounting of accessories; these are bound to be important points in commercial work, due to the necessity of wireless and other forms of electrical generating apparatus being driven by the engine. The engine itself is not particularly accessible. It is difficult to mount it on an airplane and it does not compare at all favorably in this respect with the water-cooled engine. The air-cooled engine is not well adapted to being overhauled and reassembled. It is of the porcupine type. There is nothing to set it down on. It has things sticking out in every direction and must be suspended in order to take it apart successfully. Breakage of the very light fins, which are essential to the operation and light weight of these engines, is a common occurrence if they are not handled with extreme care, and part of the cooling effect is lost when the fins are broken.

When the cooling, mean effective pressure and various tests of the kind that have been made on single-cylinder engines are done with a full knowledge of what multi-cylinder construction means, the form of test is very edifying, but may lead to great misconceptions. The single cylinder is ideal from every viewpoint regarding mixture-strength and similar considerations, and everything indicates that the correct mixture-strength has more to do with the low temperature of the exhaust-valve and the cylinder-walls than anything else.

It is easy to reach these conditions in a single-cylinder engine; in a multi-cylinder engine it is not. Unless they are inside the safe limit of cooling, engines that run successfully with a single cylinder are apt to show serious heating difficulties in one or more of the cylinders when put in a multi-cylinder form with its inseparable differences of distribution.

The problem of parasite resistance is still undetermined. One thing which must be decided before going into that matter is what we are to do with the exhaust. I should have mentioned this in regard to the radial air-cooled engine; it is a bad engine from which to handle the exhaust. Part comes from an ideal place below the machine and part directly into the face of the pilot, unless the engine has a manifold. If it has a manifold, the parasite resistance is greatly increased. If the engine is cooled properly and has a fairly large diameter, the pilot's view in the most important directions, forward and downward, is about as bad as it could be. It is difficult to comprehend how a pilot can be mounted behind a 50-in. disc and see much in the direction he is traveling. This is an important feature, especially in military machines, and it seems worth considering in commercial machines. As no one has operated large, air-cooled engines successfully in any other form, I think we must consider the radial form as possibly the only successful form at present where power results approaching those of water-cooled engines can be expected. For that reason, we have a right to criticise this form in that respect.

We do many things in different ways. Sometimes we try to make one part do two things and at another time we find it is better to have two parts, each particularly suited to its own work. In the indirect cooling of the water-cooled engine, the radiator is designed only for maximum cooling and minimum head-resistance, which can be accomplished. If the radial engine is restricted from the manufacturing point of view, the fins cannot be put on in the proper direction. This is probably not at a right angle to the central line of the cylinder; it might be better if they were put on at an angular slope in respect to this line.

In the matter of cooling, the airplane designer should be willing to sacrifice head-resistance. That is true with the cooling of the rear of the cylinder, which produces the serious parasite resistance. I have seen photographs of many of the mountings of these powerplants and do not believe for a moment that the cooling of the rear of the cylinders as shown in those photographs is properly taken care of at all. There is an attempt to streamline the cylinders from the back, to prevent the turbulence of the air behind the cylinders that is necessary for proper cooling.

MR. HERON:—Evidently Mr. Crane does not grant the air-cooled engine a single advantage over its rival, and finds only ground for not altogether well informed or unbiased criticism. Although it is granted that the Hispano-Suiza engine was undoubtedly the most generally useful to the Allies, yet in the British army the air-cooled, stationary, V-type engine rendered much good service to the cause and required much less maintenance. Most of the necessary maintenance could be done without removing the engine from the airplane. The single-cylinder-engine test certainly does not represent air conditions, but something much more severe. Cooling air-blast velocities are almost always lower than those obtained in the air, and all the British figures and the later figures of the engineering division of the United States War Department represent results maintained on continuous full-throttle at ground level.

C. B. DICKSEE:—Mr. Lawrance's paper forms a valuable addition to the information available on the subject of air-cooled engines. The air-cooled engine offers many advantages to the automotive as well as to the aeronautical engineer; it provides one solution of the fuel problem, as it will handle the heavy ends without difficulty and, when it is heated-up, if provided with a short and direct manifold, will run on kerosene without a vaporizer.

When mentioning cylinder temperatures, the mixture-strength must be taken into consideration, as this has a very great bearing on the actual temperature recorded. As an illustration of this, some figures taken from a cylinder of 3⅜-in. bore and 5-in. stroke, running at 1400 r.p.m. with an axial air velocity of only 1750 ft. per min., are of interest. They are given in Table 1. Various curves also are shown in Fig. 8.

The airflow being axial, the temperatures around the working barrel were very uniform, there being only a 70 to 80-deg. fahr. maximum difference at the upper end and less than 30 deg. fahr. difference at the lower end.

Greater differences, of course, existed on the combustion-chamber. The cylinder was of cast iron fitted with 60 sheet-steel fins, 0.05 in. thick and 0.75 in. high, cast in, so as to produce a perfect weld. This provides a very satisfactory cylinder for moderate outputs. The engine not being intended for high duty, no attempts have been made to obtain high outputs. The air velocity used would, probably, be too low for continuous operation at the output recorded. There was, however, no sign of detonation. The mixture-strengths were obtained by converting exhaust analyses taken with an Orsat apparatus into pounds of air per pound of fuel by Dr. Watson's combustion chart. The last column of Table 1 will answer Mr. McKenzie's question as to heat carried away by the cooling air. The actual figure will vary with the power output and air-velocity as well as mixture-strength, just as it does in water-cooled engines; but the values are of the same order.

It is possible that the lack of satisfaction Mr. Lawrance obtained with a bolted-on head was due not to the fact that the aluminum was not carried well down the side of the combustion-chamber, but to the distortion produced in bolting-down. Distortion of the valve seats is very easily produced in this way and any leakage will produce excessive temperatures. An increase in temperature of 250 deg. fahr., due to a very slight leak in the spark-plug gasket has been noticed. If Mr. Lawrance will replace the few large bolts with a greater number of small ones, his difficulties probably will disappear. The British Air Board reports having produced a satisfactory cylinder of this type, the size being, I think, 6½ x 8 in.

There is no mystery as to the quantity of heat that can be taken care of by any given fin. If the thermal conductivity of the material, the dissipation constant for the surface, the velocity of the air and its temperature difference from that of the surface are known, the quantity of heat dissipated by any given fin can be readily calculated.

MR. HERON:—It has been found that for aluminum-and-steel cylinders of the type shown in Mr. Lawrance's paper, the maximum cylinder-temperature is developed at the best compression-ratio on an air-fuel ratio of 12 or 13 to 1 by weight. Mr. Dicksee mentions calculating fin sizes and sections. Much useful work can be and has been done along these lines; however, the theoretically correct fin, particularly when made of high-conductivity material, is of such section that it cannot be produced commercially and, even if it could be, it would be excessively fragile. He attributes most of the unsatisfactory performance of the bolted-on detachable-head cylinder to distortion. The R.A.E. 21T 5½ x 6½-in. bolted-on head cylinder is cited as an example of a successful bolted-on head, but this is not intended to be detachable in the generally accepted sense. No evidence that I know of has shown that the power output or fuel-consumption of this cylinder has proved as good as that of similar types with cast-on or screwed-on heads.

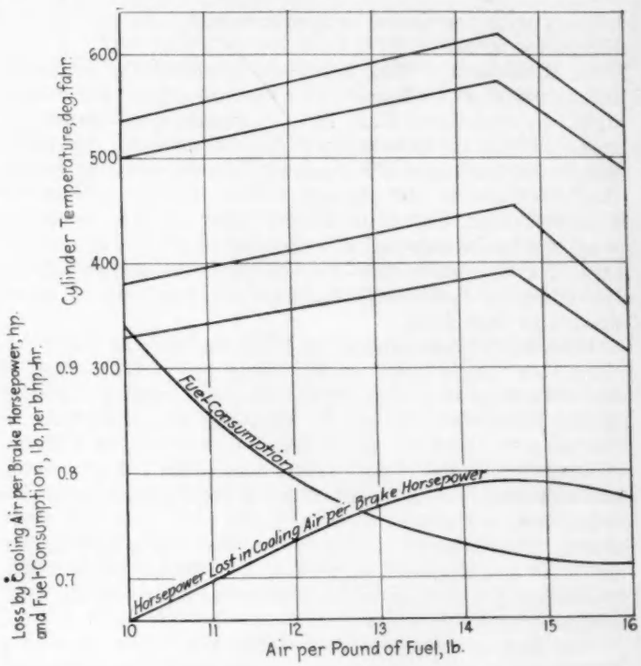

FIG. 8—RELATION BETWEEN CYLINDER TEMPERATURE AND MIXTURE-STRENGTH IN A 3⅜ X 5-IN. AIRCRAFT ENGINE RUNNING AT 1400 R.P.M.

Tests by the engineering division of the War Department on two cylinders of composite aluminum-and-steel construction, and of identical design, except that one has a cast-on head and the other a bolted-on detachable head, have shown the bolted-on head to be inferior as regards output, fuel-consumption and temperature developed. The bolted-on head is liable to leak gas at the joint and, if the flame gets between an aluminum head and a steel or cast-iron barrel, the former is usually burned out rapidly. Axial heat-flow from the head to the barrel is practically non-existent with a bolted-on head, and experience indicates that such heat-flow is important.

A design in which all heat given to the head must be dissipated by that part seems inferior to a type where heat flow is possible and the barrel is allowed to dissipate part of the heat given to the head. A screwed-on head of the type used by the engineering division has much more cooling-fin surface integral with the head than will exist with a detachable head in which the head ceases and the barrel starts at the top of the piston travel; and considerable heat-flow from the head to the barrel is possible in view of the fact that the surface of the head in

TABLE 1—RESULTS FROM 3⅜ X 5-IN. CYLINDER AT 1400 R.P.M.

Mixture-Strength, Air per Pound of Fuel, lb.	Brake Horsepower	Average Temperatures, deg. fahr. Top of Head[e] (A)	Side of Combustion Space[e] (B)	Working Barrel[e] Top (C)	Working Barrel[e] Bottom (D)	Fuel-Consumption, lb. per b.hp-hr.	Loss by Cooling Air per B. Hp., hp.
10	5.10	535	500	380	330	0.940	0.660
11	5.10	554	513	392	342	.....	.....
12	5.10	575	530	410	356	0.795	0.735
13	5.10	592	548	424	367	.....	.....
14	5.00	612	570	442	382	0.736	0.790
15	4.65	594	548	410	358	.....	.....
16	4.25	530	491	360	316	0.710	0.775

[e]Letters correspond with those on the temperature curves in Fig. 8.

contact with the barrel is approximately 130 per cent of the piston area.

D. R. HARPER:—The results of some of the measurements made at the Bureau of Standards may throw some light on questions that Mr. Lawrance has mentioned more or less in speculation. Almost no data regarding the head-resistance of air-cooled cylinders are as yet in the literature of the subject. The suggestion that the head-resistance is rather higher than is often supposed must not be interpreted as condemning the air-cooled engine, for the latter has its field and, regardless of the loss of power due to head-resistance, it may be very much needed in that field.

Whenever, in designing, we combine two distinct functions in a single part, we are likely to sacrifice some of the effectiveness of both functions. The cooling capacity of the air-cooled cylinder is naturally less than that of the water-cooled one, where the power functions and the cooling functions have been separated. The water radiator has been brought to a wonderful degree of development, while air-cooled cylinders, in spite of their ability to utilize a much greater temperature-head, are seriously handicapped on account of offering more head-resistance for the same cooling power than do the well designed water systems.

The measurements, which form the basis of these statements, were wind-tunnel experiments in a series, made by the Bureau of Standards for the engineering division of the Air Service. The air-cooled cylinders or radiators were mounted in a free-air or "unobstructed" position. When we consider the air-cooled engine as mounted in a fuselage, there are mutual interference effects of the several cylinders and modification of the air-flow by the fuselage, so that the figures given would be changed; whence, caution must be exercised in drawing general conclusions. The figures are of interest in suggesting the order of magnitude involved. In the free-air position, a single-cylinder assembly of each of the air-cooled engines offered a head-resistance about 30 per cent greater than that of an equivalent water radiator. The cylinders used were typical of the late-war and post-war eras and, although including a Lawrance aluminum cylinder, were mostly types now considered obsolete and not worthy of the expenditure of the time required for careful investigations. The measurements were distinctly of preliminary character and no great significance attaches to the figure of 30 per cent. An analysis of the contributions to head-resistance by various parts of an engine was then made to see whether appreciable savings in windage might be effected by changes in design.

The measurements were along the lines indicated by Mr. Lawrance in mentioning the crusader cowl, and afford quantitative data for consideration in designs of this nature. The results obtained proceed from observations made with sufficient care to warrant publication and, as I believe a report will soon be printed, I will condense the account of the work to a minimum. Working with a large wind-tunnel at the Bureau of Standards, Miss L. Butler and C. F. Zobel measured the head-resistance of a Lawrance cylinder-assembly in successive stages of stripping-off valve push-rods, rocker-arms, springs, ignition cables, intake tubes and the like. The results indicate the contribution to head-resistance of each part, mounted in its relative position, with the cylinder as a whole in a free airstream. The total contribution by parts thus removable was 16 per cent of the total head-resistance of the complete cylinder. If, therefore, parts that need not be in the airstream because they contribute little or nothing to the cooling could be enclosed in a perfect cowl or zero resistance, the total saving in windage would be about 16 per cent.

G. J. MEAD:—In commenting on Mr. Lawrance's paper, I suggest that we look for a moment at the way in which radial and water-cooled engines of equal power compare when measured by what might be called the five prime requisites of an aviation engine, as considered in the following paragraphs.

It must be granted that the reliability of a radial engine is at least equal to that of a water-cooled type, especially as the same accessories and fuel system are used with both. As we all know, it is usually the accessory systems that cause failure in the air, and not the engines themselves.

Performance includes two items. The first is the effective power required for take-off and for satisfactory operation at working altitudes, with the specified fuel. The effective power, or power available for moving the airplane over and above that required to propel the powerplant, is probably less in the case of a radial engine than in that of a water-cooled engine with radiator, especially in large engines. This is a question that must be settled by actual flight tests. It is hoped that before long the same airplane will be flown with both types of powerplant, each having the same power at sea-level. The cause of possible reduction in effective power is the larger head resistance of a radial engine. Radial engines excel slightly, perhaps, in the second item of performance; that is, low powerplant weight per horsepower per hour. The elimination of water, radiator and piping gives them an advantage, even though comparatively low mean effective pressures are developed.

The durability of radial engines in service has yet to be proved. If care is used in proportioning the bearings and adequate lubrication is provided, it is doubtful whether failures of these parts will occur. On the other hand, I question seriously the ability of the valve-gear to stand up, owing to the large clearances between the cams and the valves that develop in operation on account of the expansion of the cylinders. It should be borne in mind also that aluminum cylinders with thin aluminum fins are not durable. If the fins are really necessary for satisfactory cooling, the efficiency of such cylinders in service is bound to drop.

Our experience with air-cooled engines has led us to believe that failures are due to the breakage of valves and springs. It seems to me that the work done in England on the Cosmos engine, now known as the Bristol, is a good indication that this is so. It shows an effort on the part of the designer to make use of a type of valve gear intended to eliminate this difficulty. It seems that the push-rod-operated valve will be used for some time to come.

Air-cooled cylinders of cast-aluminum construction are easily damaged. Mr. Crane mentioned the porcupine type. If the cooling of such cylinders is due to the fins, the efficiency will be less if many fins are broken off.

Most of the requirements of an airplane powerplant, such as being readily mounted, easily started and accessible, are met equally well by the radial and the water-cooled types. Proper vision, especially in military types of aircraft, is difficult with radial engines, and is apt to become a serious matter with large engines. Due to the compactness of the powerplants, the maneuverability of craft equipped with radial engines probably would be better than those having water-cooled engines. Radial engines are but little more compact than water-cooled, eight-cylinder, V-types. The performance of an engine in flight, throughout its speed-range and at different alti-

tudes, is an important factor in plane requirements, and involves the necessity of temperature control for the engine cylinders. No satisfactory method of shuttering radial-engine cylinders, as is done with radiators, has been evolved, so far as I know. Owing to the great conductivity of aluminum, which makes the present-day radial-engine possible, it is believed that the engine will cool-down very rapidly in a glide. For military work, this drawback must be overcome before the engine will be entirely practicable. Smoothness of working and flexibility are apt to be seriously affected by lack of proper temperature control.

The first costs of a radial engine and of a water-cooled engine, when they are produced in equal quantities, would undoubtedly be about the same. The cost of upkeep will depend to a great extent on the durability of the engine. This question, as already pointed out, has yet to be settled by service tests.

Summing up the above analysis, we find that radial engines have yet to be tested in service to prove how they compare with the best water-cooled engines with respect to three items: effective power, durability and controllability of the engine under service temperatures. The Wright Aeronautical Corporation is developing for the military authorities a large radial engine with 5⅝ x 6½-in. cylinders, and a displacement of 1453 cu. in. Two of these engines have been built and the first one has been running for some time. The maximum power developed was 350 hp. at 1800 r.p.m., with a mean effective pressure of 106 lb. per sq. in. This, so far as I know, is the largest radial engine which has been successfully operated in the United States. New cylinders are being developed which give a promise of 120 lb. per sq. in. mean effective pressure. These will permit the engine to develop 350 hp. at 1600 r.p.m., or 400 hp. at 1800 r.p.m. The final engine of this type will weigh between 750 and 800 lb., which will be about 2 lb. per hp. It is interesting to note that the best figures for a water-cooled engine, including radiator and water, are 2.2 lb. per hp.

I believe that radial engines will fulfill certain requirements satisfactorily, particularly those of some kinds of military service. Their advantages for all purposes have not been demonstrated and until service tests of sufficient duration to settle the doubtful points shall have been conducted, no one is justified in considering them beyond the experimental stage.

R. W. A. BREWER:—This controversy between air-cooled and water-cooled engines appears to hinge on the suitability of the mounting of the air-cooled engine. I believe the air-cooled engine can be made as efficient as the water-cooled engine. F. W. Lanchester's papers before the Institution of Automobile Engineers contain much matter on air-cooled engines that is extremely valuable.

I used a machine with an Anzani engine for experimental work in 1909, just after Bleriot flew across the English Channel, and also one of the first Gnome engines. In those days, although the air-cooled engine was young, the Wright engine was partly air-cooled; its cylinder-head was air-cooled. We got an overall efficiency with the Gnome and its propeller that was comparable with that of any other type of engine, for the reason that propeller flutter was eliminated. The periodic vibrations of the crankshaft, a serious factor in the efficiency of propellers, did not enter. By eliminating propeller flutter we increased the efficiency of the propeller and made up for any small loss due to engines of this type. Somebody has referred to the cylinder distortion caused by the difference in temperatures at the front and the back of the cylinder. The piston-ring of the Gnome engine was one of its interesting details. It was an L-shaped, gun-metal ring held out to the cylinder barrel by a steel ring. There might be distortion and yet the ring would conform to the bore of the cylinder.

CAPT. GEO. E. A. HALLETT:—Airplane designers in the engineering division apparently consider the head-resistance of radial engines to be higher than that of the best water-cooled engines. While the plan is not to use radial engines in planes of extremely high speed, from a military viewpoint the air-cooled engine, of radial or any other type, is advantageous. A large percentage of forced landings is due to troubles in the cooling system, such as cracked water-jackets, radiator leaks, loss of water through evaporation or boiling and in some cases

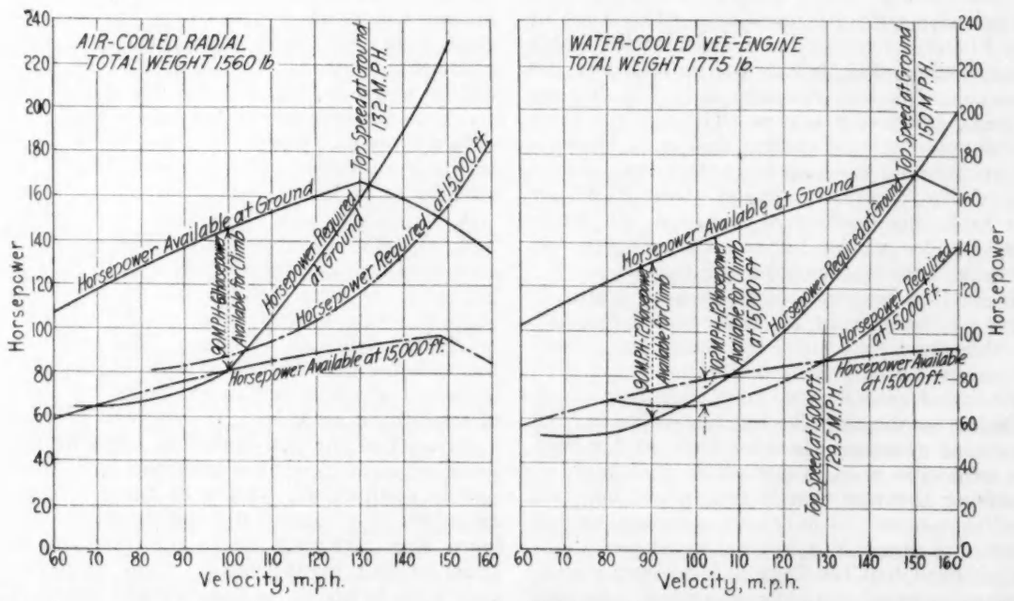
AIR-COOLED RADIAL
TOTAL WEIGHT 1560 lb.
Top Speed at Ground 132 M.P.H.
Horsepower Available at Ground
Horsepower Required at Ground
Horsepower Required at 15,000 Ft.
90 M.P.H. Horsepower Available for Climb
Horsepower Available at 15,000 Ft.
Horsepower
Velocity, m.p.h.
WATER-COOLED VEE-ENGINE
TOTAL WEIGHT 1775 lb.
Top Speed at Ground 150 M.P.H.
Horsepower Available at Ground
Horsepower Required at Ground
90 M.P.H. Horsepower Available for Climb
122 M.P.H. Horsepower Available for Climb at 15,000 Ft.
Horsepower Required at 15,000 Ft.
Horsepower Available at 15,000 Ft.
Top Speed at 15,000 Ft. 129.5 M.P.H.
Horsepower
Velocity, m.p.h.

FIG. 9—COMPARISON OF THE LIBERTY ENGINE WITH AN AIR-COOLED RADIAL TYPE AS REGARDS POWER AND SPEED AT VARIOUS COOLING-AIR VELOCITIES

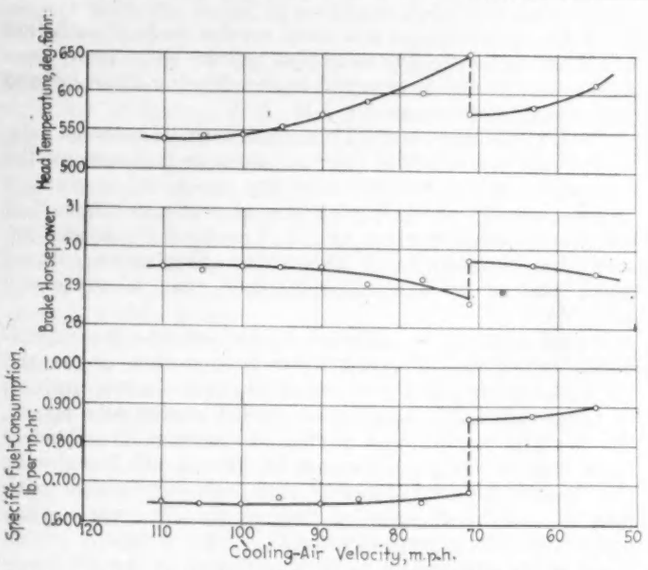

FIG. 10—RESULTS OF A TEST OF A 5½ X 6½-IN. AIR-COOLED CYLINDER MADE BY THE ENGINEERING DIVISION OF THE AIR SERVICE

even freezing. We feel that we are thoroughly justified in developing air-cooled engines for military purposes for this reason, and also because they are less vulnerable to machine-gun fire.

Some interesting reports of brake mean effective pressures and fuel-consumption in English engines have caused us to be skeptical because we have never seen them checked up. Recently Mr. Heron, who worked for years with Dr. Gibson at Farnborough in England, has designed a cylinder which we have constructed and which has given excellent results. Both Mr. Lawrance and Mr. Mead have referred to this cylinder. Not only has it shown a brake mean effective pressure of more than 130 lb. per sq. in., and a capacity for going higher, but it has done this on a fuel-consumption as low, in some cases, as 0.52 lb. per b. hp-hr. This compares favorably with water-cooled cylinders running at a similar compression-ratio, and we believe it is able to do better.

The most interesting thing about this two-valve cylinder has been the cooling of the exhaust-valve. It is a 5⅝ x 6½-in. cylinder with a compression-ratio of 5.4 or 5.5 to 1. The Liberty single-cylinder engine with which I make a comparison in Fig. 9 is 5 x 7 in. and uses the same compression-ratio, but develops only 120 lb. per sq. in. brake mean effective pressure. The exhaust-valve in the air-cooled cylinder runs cooler; that is, it shows a wider black band around the seat than does the Liberty valve, in spite of the former running at about 10 lb. per sq. in. higher brake mean effective pressure. In short, the air-cooled cylinder shows better exhaust-valve-seat cooling than the Liberty water-cooled cylinder does.

It seems to me that we are failing to keep water in contact with the cylinder-head in water-cooled engines. We have a bubble sheet that holds the water away from the cylinder-head pretty effectively while developing high powers. In air-cooled cylinders we have no bubble sheet. We may get better cooling of the exhaust-valve in well developed air-cooled cylinders than we have so far been able to obtain with thin steel, water-cooled cylinders.

We found during the war that it took about one-half the number of man-hours to overhaul a rotary engine that was required to overhaul a V-type, water-cooled engine, because we used ball bearings and no long crankcase with its troubles in aligning the crankshaft bearings and had no water connections. While it is true that the radial engine may need to be handled more carefully than the water-cooled engine, it has the advantages of ball bearings and a short crankshaft. We cannot make a very good job of overhauling a rotary engine without special tools developed for such work, but those can be developed also for the radial engines.

C. F. TAYLOR:—Mr. Lawrance has presented an excellent paper covering the development of air-cooled aviation engines. One point concerning which a fuller discussion would be of interest is the marked effect of the quality of the mixture on the performance of air-cooled cylinders. The data given on specific fuel-consumption for the various cylinders and engines do not represent the lowest consumption possible, but rather the lowest consumption at which it is considered practicable to operate the engine in question. It should be remembered that the chief controlling factor in fuel-consumption is the quality of the mixture fed to the cylinders, and that even cylinders of poor design can be made to show a surprisingly low specific fuel-consumption if supplied with comparatively lean mixtures.

In air-cooled engines of exceptionally good design, the practical low limit of fuel-consumpton, as in the case of good water-cooled cylinders, is the point at which the power output begins to decrease on account of insufficient fuel to utilize all the air in the cylinder. In most air-cooled cylinders, however, the power decreases on account of excessive value and head temperatures before the point of insufficient fuel supply is reached. This is because the average air-cooled cylinder depends to some extent on cooling by the excess fuel in a rich mixture, to keep the combustion-chamber and exhaust-valve temperatures within practical limits.

The important part that excess-fuel cooling may play in a cylinder of mediocre design is illustrated in Fig. 10, which shows the results of a test by the engineering division of the Air Service, on a 5½ x 6½-in. air-cooled cylinder of aluminum and steel construction. The cylinder was tested at full power with a mixture-ratio giving a specific fuel-consumption of 0.65 lb. per b.hp-hr. and the air-blast speed was reduced gradually from a maximum of 110 m.p.h. It will be noted that the cylinder-head temperature, taken at the hottest point, rose steadily as the air speed was reduced. At an air speed of 71 m.p.h., the head temperature was considered dangerously high and the mixture ratio was adjusted to give a specific fuel-consumption of 0.87 lb. per b.hp-hr. As will be noted in Fig. 10, this restored the power output and head temperature to the values that formerly existed with a 90-m.p.h. blast. In other words, a small increase in the gasoline-air ratio was as effective in cooling the cylinder as an increase of 20 m.p.h. in the blast speed.

A. L. CLAYDEN:—There appears to be an assumption that, although the direct air-cooled engine may be in its infancy, the water-cooled engine is in its old age; but the latter is not true. First, we do not know everything there is to know about water-cooled engines and, second, we do not utilize all we know. The weights of the water, the radiator and the water connections are considerably in excess of what they might be if we went to the limit of everything we know. It is a very surprising fact that a majority of the new machines being designed today for commercial purposes are provided with inefficient water-cooling equipment. I know of many instances where the designers of airplanes did not appear to appreciate that there was anything worth studying in regard to the water-cooling arrangement. One subject about which very little is known is the rate of transfer of heat from a cylinder to the water. It is believed that by narrow-

ing the jacket and increasing the rate of flow through it, designers could reduce very substantially the weight of water and to some extent the weight of cylinder, obtaining at the same time a mean temperature in the cylinder with a narrower range between the extremes than those at present. In other words, the temperature of the cylinder in water-cooled engines will vary between 400 and 240 deg. fahr., or something on that order. Under the best possible conditions and with properly designed water-passages, that range undoubtedly can be reduced so as to bring the maximum to about the boiling point; although I do not know whether that will have any particular importance.

I noticed a test recently that was rather interesting. The normal temperature at the exhaust in an automobile engine was about 240 deg. fahr. As soon as steam was observed in the outlet water, the temperature of the exhaust-valve rose to over 400 deg. fahr., an almost instantaneous rise that merely indicated where the steam was. If the air-cooled engine really begins to push the water-cooled engine hard, the water-cooled engine advocates have plenty of resources.

H. S. McDEWELL:—As I work with both water-cooled and air-cooled engines, I hold no brief for either type. I wish, therefore, to bring out some points that Mr. Lawrance has not mentioned, and to amplify some of his statements.

In speaking of heat transfer, from hot gases in the cylinder to the cooling system, Mr. Lawrance has omitted some of the steps. These are indicated in Fig. 11; for a cylinder constructed with a single thickness of metal at the left, and for a cylinder built-up of two metals, at the right. The lines bounding *b*, at the left, represent the cylinder wall. The horizontal and sloping lines denote the temperatures existing in the various parts of the system, measured from an assumed datum, or zero-temperature line. It is known that in a transfer of heat from a gas to a fluid of any type through a metal there is a great temperature-drop in the fluid films that lie close to the metal surfaces. These films are represented by the lines bounding *a* and *c*. The drop through the hot, dead-gas film *c* will be somewhat of the order indicated by $t_1 - t_2$. The temperature-drop in heat transfer through the metal *b*, will be of a very much lower order, as indicated by $t_2 - t_3$. Again, in transferring heat from the metal to a fluid, there will be another dead film *c* and again there will be a very serious temperature-drop as indicated by $t_3 - t_4$, to determine the mean temperature of the cooling medium t_4. In an air-cooled engine, the three necessary steps are represented at *a*, *b*, and *c*; *a* being through the first gas film, *b* through the metal of the cylinder and *c* through the second dead-gas film to the cooling medium. In a water-cooled engine we have another metal surface in the radiator and another drop through the dead film, a very small drop through the metal and a further drop through the second film to the temperature of the cooling medium. Therefore, there are twice as many transfer-steps in the process of water-cooling as obtain in air-cooling.

I object to one statement by Mr. Lawrance, that the water-cooled engine is handicapped by the number of heat transfers, all of which tend to increase the ultimate temperature on the inside of the cylinder. Previous to this Mr. Lawrance demonstrates that the temperatures on the outside of the cylinder wall of both air-cooled and water-cooled engines are approximately the same. Taking the case of the Hispano-Suiza engine, we have a

[7] See S.A.E. TRANSACTIONS, vol. 16, part 1, p. 577.

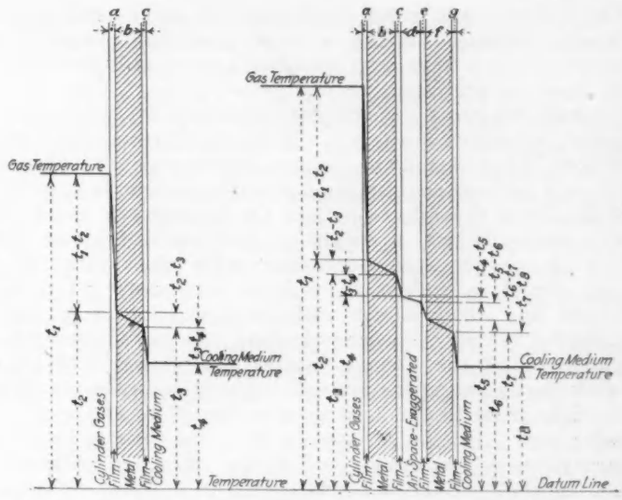

FIG. 11—DIAGRAM ILLUSTRATING THE TRANSFER OF HEAT FROM AN AIRCRAFT-ENGINE CYLINDER TO THE COOLING MEDIUM

transfer through two metals, with an infinitesimal gap between, no matter how perfect the manufacture may be; there is thus an unavoidable lack of thermal bridge when such a construction is involved. Consequently, as shown at the right in Fig. 11, we have an additional drop in temperature through *d*, a still further serious drop through *e*, another small drop through the second layer of metal at *f* and a final drop through the film *g*. If the cooling-temperatures t_4 and t_8 are the same in both the types of construction indicated in Fig. 11, it would be reasonable to presume, from heat-transfer considerations, that the temperature inside the cylinder would be less in the case of a single thickness of metal. Granted that the air-cooled cylinder has been designed in which the temperatures inside and on the outer wall are comparable with those obtaining in water-cooled cylinders, the mean temperatures of the cooling medium due to these heat-transfer considerations must be approximately the same irrespective of the medium. The fact that water boils at 212 deg. fahr. at sea level is not really a handicap in operation, because we must decide upon the safe limit of temperatures of engine parts and stay within it, presuming a properly designed water-cooling radiator system.

Regarding the matter of handling engines while being overhauled one thing must be kept in mind; that is, that, while the factory crew may be well trained, both Army and Navy engines may be sent to some far-distant station which is not equipped with special apparatus. To have doughboys handle a porcupine engine during its overhauling is a serious proposition.

In considering the comparative resistance of the air-cooled and the water-cooled engine, I refer to Grover C. Loening's paper on Engine Shape as Affecting Airplane Operation.[7] He made a good comparison between water-cooled and air-cooled engines and pointed out that the question of relative weight is of minor consideration and that head-resistance is the primary factor. Most persons speaking in favor of the air-cooled as against the water-cooled engine refer to the radiator as a nose radiator. The nose radiator is obsolete. It stands to reason that if a radiator is mounted in front of an engine, the area of that radiator must be increased enough to compensate for the obstruction that is at the back of it in the form of the engine. The latest and best practice in radiator installation in planes is to put the radiators in the clear, not only in the mounting but in the actual construction.

There have been great improvements lately and, as a result, radiators having a very materially increased figure of merit have been produced and actually installed in some powerplants.

I have refigured a portion of Table 2 in Mr. Loening's paper, comparing the Curtis CD 12, the Hispano-Suiza or Wright Model H and the Cosmos-Jupiter engines, assuming as a basis of comparison a propeller weight in pounds of 2.5 times the square root of the horsepower. I have also assumed first, a weight of radiator and water of 0.65 lb. per hp., in agreement with older practice; and, second, a weight of radiator and water of 0.56 lb. per hp., which agrees with later practice using the improved radiators mentioned above. In both cases the resistance of the Cosmos-Jupiter engine was reduced below that given in the original table, in accordance with the data given by Mr. Lawrance on the effect of the crusader cowl. On the first basis the total resistance per horsepower of the engine and of the retractive radiator and the drift of the wing necessary to lift that powerplant, figure out to be practically identical for the three engines at 200 m.p.h. When the data for the better types of later radiator are included, the figures favor the water-cooled engine.

One of Mr. Lawrance's engines had completed all but 4 hr. of the 50-hr. test when I left Washington, and the latest data from it happen to be slightly better than the figures quoted by Mr. Lawrance. Running at 1775 r.p.m., the actual power developed was 228.7 hp. Corrected for barometer and temperature, it developed 217.7 hp., which gives an actual mean effective pressure of 129.5 lb. per sq. in., or a corrected mean effective pressure of 123.3 lb. per sq. in., which is high. The fuel-consumption was 0.58 lb. per b.hp-hr. and the oil-consumption was exceedingly low, being 0.013 lb. per b.hp-hr. The reason for the high fuel-consumption is that, due to the Navy program, this test was run with the utmost expedition to develop what the engine would actually do in the matter of reliability; it was not tested for economic performance, but to develop the weak structural points. Consequently, very little attention has been paid to carburetion. I have no doubt that the fuel economy could be improved. Another factor is that the carbureters on this engine were designed specifically for a very low fuel-consumption at cruising speeds, rather than a necessarily low consumption at full-throttle. At 0.9 of the rated horsepower, we have shown a fuel-consumption of about 0.51 per lb. per b.hp-hr.

I wish to bring to the attention of those who are working on air-cooled engines the same liability to trouble that is met in the case of water-cooled engines; that is, the possibility of trouble at either altitude or ground-level under cold, damp conditions, due to the formation of ice in the manifold. The water-cooled engine has the defect that the water-jacketing on the manifold is not entirely satisfactory. A greater amount of heat than is available in the water is necessary to prevent the formation of ice absolutely.

H. E. MORTON: — The comparison of air cooled with water-cooled engine is always an interesting subject. The development of the air-cooled engine during the last few years has been very remarkable. Mr. Lawrance refers to the several heat-transfers in a water-cooled engine as producing a handicap in comparison with direct air cooling. I question whether actual temperature measurements would confirm this assumption. In either case, of course, we would consider that the design was such that all parts of the inside cylinder-wall were uniformly cooled, whether directly or indirectly.

The transfer of heat from the gas through the metal into the water is controlled almost exclusively by the transfer from the gas to the metal, as the water will take up many times as great a quantity of heat per unit of surface. Similarly, with the radiator, the water will transfer to the metal many times more heat than can be absorbed by the air. The water in an engine jacket is, of course, merely a carrier and an exceedingly efficient one; so, in my opinion, assuming other things to be equal, the number of transfers of heat in the water-cooled engine does not enter into the problem.

Mr. Lawrance cites later the limitation of a water-cooled engine that the cooling medium boils at 212 deg. fahr. This is certainly a limitation at present, but I am anxious to see the development of a satisfactory cooling system using a liquid that boils at nearly 300 deg. and yet does not freeze at a temperature above 0 deg. fahr. Kerosene meets these requirements so nearly that I believe it would be widely used, provided the engine cooling-systems were designed with the idea of handling it.

A MEMBER: — How does Mr. Lawrance measure the cylinder temperature? Does he use a thermocouple? If so, what precaution did he take to measure the particular point in which he was interested? Was the cylinder shown in Fig. 13 of his paper an aluminum, an iron or a steel cylinder? Have any figures been compiled for the relative temperatures under corresponding conditions of an aluminum and a steel cylinder? What method did Captain Hallett use in measuring the temperatures of air-cooled cylinders?

MR. HERON:—The temperatures on the Airco cylinder given in Fig. 13 of Mr. Lawrance's paper were measured by platinum-iridium thermocouples of the contact variety. This type of couple measures point temperature and the accuracy is probably within minus 20 deg. fahr. on the head temperatures and minus 50 deg. fahr. on the barrel readings. The relative accuracy of head readings to each other, or barrel readings to each other, is within 20 deg. fahr.

Regarding the relative performance of steel and aluminum cylinders, no figures are known of such tests on cylinders of similar design. Tests were, however, carried out on cast-iron and aluminum cylinders of almost identical design to that shown in Fig. 8, but fitted with shrunk-in liners, such as are provided for in Fig. 7 of Mr. Lawrance's paper. The cast-iron cylinder, cast from the same pattern as that for the aluminum cylinder, developed the same brake mean effective pressure as the aluminum type, but on a 10-per cent higher specific fuel-consumption. Reference is made to a Resume of Experimental Work on Air-cooled Cylinders at the R.A.E., by A. H. Gibson, D.Sc.[8]

MR. LAWRANCE:—The information given in Fig. 13 of my paper was obtained from S. D. Heron of McCook Field. The cylinder shown in Fig. 13 has an aluminum head and a steel body. I am sorry to see any one so pessimistic as Mr. Crane is about the future of air-cooled engines. I believe he will modify his views in the course of time. I cannot agree with his statements regarding rugged simplicity. The simplicity of a crankshaft that has nine bearings as opposed to one having three does not appear very great; and water pipes that must be made so that the flow of water to each cylinder will be perfect, to cool the exhaust-valves of each cylinder properly, are not a simple matter either. What Mr. Crane says about single-cylinder water-cooled engine tests is perfectly true. With air-cooled engines it is not true. In an air-

[8] See *Advisory Committee for Aeronautics Report* No. 24.

cooled engine there will be just as good air-flow around the heads and valves of each cylinder, no matter what the number of cylinders may be. In the water-cooled engine, if the water pipes are not arranged right, or some eddy occurs in the flow, one cylinder will be overheating while another is cooling properly. For that reason a multi-cylinder water-cooled engine will not come up to the standard of a single-cylinder water-cooled engine.

Another point about these so-called fragile, delicate air-cooled engines is that, as has been said, a crew of men who are accustomed to handling those engines very rarely breaks even a fin, although this does occur occasionally. The most damage occurs where aviators run into things with a plane; generally, in that case, a water-cooled engine also suffers somewhat. If an Hispano-Suiza engine is dropped on the ground, the water-pump will generally break. They must be handled carefully. Of course, if a few cooling fins break off, it does not matter much, but, if the water-jacket is broken, within a few minutes the engine will not operate.

One can remove the cover from our nine-cylinder engine and take out the crankshaft by removing one cylinder and unbolting the master rod. Any cylinder and connecting-rod can be taken out without removing the crankcase from the airplane. The arrangement is such that one can clean the screens for the strainers through a very conveniently located hole at the bottom of the engine. It is not necessary to remove the carbureter to take the engine off the fuselage; it is necessary only to break the oil pipes and remove the bolts that hold the engine to the bulkhead. Those who investigate modern air-cooled engines will find that they are rugged, simple and easy to maintain.

MR. MEAD:—What is the necessity of temperature control of air-cooled cylinders in flight?

MR. HERON:—The necessity of temperature control of air-cooled aircraft engines has yet to be proved, at least as applied to the cylinders. Efficient air-cooled cylinders will operate satisfactorily over a wide range of temperature. Maintenance of more or less constant carbureter-temperature conditions is undoubtedly desirable and in this connection Captain Hallett has suggested that carbureters be situated in the airblast, so that they will always be cold and thus adjusted for opening-up after a steep dive. The need of temperature control of an air-cooled engine is much less marked than that of a water-cooled engine. Overheating does not result in a loss of cooling medium; and a steep dive, though it makes the engine so cold that it will not open-up, does not result in damage from freezing.

Mr. Mead has raised the question of breakage of valves and springs on air-cooled engines which results from the excessive tappet-clearance developed when hot. There is no doubt that much trouble is caused by excessive clearances but, in my experience, wear rather than breakage is the usual result. It must be admitted that the valve-gear of the average air-cooled aircraft-engine cylinder is very crude. Open gears with poor lubrication and developing large tappet-clearance are not in any way on a par with high-class water-cooled-engine practice; compensating devices to secure constant tappet-clearance, whether the engine is hot or cold, overcome only one of the defects of open gears. Completely enclosing and lubricating the valves, springs, rockers and push-rods largely eliminates shock and wear and insures lubrication. The push-rods become heated, with the result that the clearance can be maintained practically constant. Maintenance of a constant clearance is of importance in avoiding an excessive change of valve-timing. Most air-cooled aircraft engines have heavily overlapped, hot-valve timing, and a large change of clearance results in an increase of an initially large overlap, usually causing the engine to start and idle poorly, and to tend to pop-back when the throttle is opened.

GOVERNMENT AID FOR AVIATION[1]

THE fundamental fault with the subsidization of air transport in Europe is that the subsidy is granted almost entirely for military reasons. It is essentially a premium on mere operation, for the most part. When new designs are encouraged, stress is put upon the military features. This is a vicious system inherently, because it is self-perpetuating; it discourages the only kind of development that can put aviation on its own feet. The European methods have been conspicuously successful for the purpose intended, but the American people has a very just suspicion on any kind of regular subsidy. I voice the majority opinion of aircraft men themselves by saying that we do not want any subsidy for the operation of aircraft in America. We do not want an industry that must be continually nursed to keep it alive. If that is the kind of business we are pushing, it does not deserve any help. But, while we may feel that some of the foreign governments are too paternalistic, we should not sit and do nothing. Our railroads never could have developed without Government cooperation. We can do at least as much for aviation.

I hardly need mention the main essentials, on which all seems to be in agreement. Some form of central governmental bureau for civilian aeronautics, such as is provided for in the Wadsworth-Hicks bill, is the most natural starting-point. Public confidence, safety of operation, the financing of new undertakings, the laying out of routes and a proper basis for insurance will all be facilitated greatly by proper Government cooperation. Let other nations back commercial aviation for its military value. If we can develop its real commercial value, military considerations will take care of themselves. A self-supporting going industry that is an economic asset to the Country is the best military asset as well. To attain this, the most serious problems at present are those of a technical nature, and these are involved almost without any exceptions in the design of the machines themselves.

France is spending $2,500,000 per year on aircraft subsidies that show every indication of being perpetual. Suppose our own Government should set aside, once and for all, one-fifth of this amount, and divide it into a series of prizes for the best new designs to accomplish the objects of safety for passengers, economy for merchandise and mail, and night flying. There is every reason to believe that, if this were done properly, the personal risk could be cut down to one-twentieth of its present value, little though it now is; that mail could be carried for a cost per pound of one-thirtieth of the present rates, successful though our air mail already is; and that aerial transportation would open up a great new field of its own not injuring present transportation systems, but supplementing them to the great advantage of the entire public.

[1] From an address before the Metropolitan Section, by Ralph H. Upson, airship designer and balloonist.

MOTOR FUEL FROM COAL

WHILE the problem of an adequate supply of liquid fuels is becoming more urgent in this Country, it may be of interest to consider how the same problem is being attacked in England where there is no natural supply of liquid fuel whatever exclusive of shale oil.

An article on The National Significance of Low-Temperature Carbonization, by David Brownlie, in the Feb. 3, 1922, number of *Engineering* (London) discusses at some length the recent developments in the production of liquid fuels and other important products by the low-temperature distillation of coal. This project was given impetus in England during the war by a keen realization of the absolute dependence of the country on outside sources of fuel for the navy, as well as for motor transport and for aviation. A systematic cooperative program of research was organized under the Department of Scientific and Industrial Research with a view to solving the technical difficulties in the production of a maximum amount of liquid fuel from coal.

The article referred to above indicates that at least some of the more serious technical problems have been solved, as well as the economic problem of a market for the products of the process. The conclusions are based on the results obtained in a plant operating on a commercial basis treating 36 tons of coal per day. This plant was described in detail in the Oct. 28, 1921 number of *Engineering*.

Mr. Brownlie gives the figures presented in Table 1 for the products of low-temperature distillates as compared with other methods.

TABLE 1—PRODUCTS FROM 1 GROSS TON OF AVERAGE COAL

	Low-Temperature Distillation	High-Temperature Distillation	
		Gas Works	Coke Ovens
Temperature of Distillation, deg. fahr.	1,000	1,800[a]	1,800[a]
Solid Product (Coal Substitutes), lb.	1,624	1,512	1,568
Nature of Product	Smokeless Fuel Coalite	Soft Coke	Hard Metallurgical Coke
Volatility of Product, per cent	0.9	1.0	0.5[b]
Liquid Product (Motor Fuel, Fuel Oil, etc.), gal.	20[c]	10	8
Nature of Product	Coalite Oil	Coal Tar	Coal Tar
Gas, cu. ft.	6,000	12,000	11,500
Calorific Value of Gas, B.t.u. per cu. ft.	700–750	550	450
Sulphate of Ammonia, lb.	15	25	28

[a] Approximate.
[b] Maximum.
[c] Including 3 gal. of motor fuel.

Mr. Brownlie points out that at present there are used in Great Britain some 140,000,000 tons of coal that could be subjected to low-temperature distillation without including that used for gas production, metallurgical coke, and similar purposes, and that the treating of this coal would produce 400,000,000 gal. of high-grade motor fuel or twice the *present consumption;* also 2,350,000,000 gal. of oil, at least 50 per cent of which would be high-grade Diesel engine fuel, and from which could be produced an adequate supply of lubricants.

This would seem to meet the present demands for all liquid fuels in Great Britain. But an equally important feature of the process is the utilization of the non-volatile residue or coke produced. The product produced by this particular process, which is termed "coalite," is said to have all the desirable qualities of coal for practically all purposes. It has the advantage of being smokeless, having only about 10 per cent volatile matter, and the author discusses at considerable length the smoke evil that might be eliminated by the universal use of this fuel. Another advantage of it is that it gives a much higher *radiant* efficiency in open grates, some 10,000,000 of which are in use in England.

A discussion is given also of the advantage of the increased production of sulphate of ammonia that would attend the general adoption of low-temperature distillation practice and could be used to increase the agricultural production of the country.

While the article seems to show conclusively the technical practicability of meeting the present fuel needs of Great Britain from the coal resources alone, and of eliminating smoke, promoting agriculture and affording a better fuel for open grates, there is no discussion of the relative cost of meeting the situation in this way as compared with the present practice of importing liquid fuels.

CORROSION OF CHROMIUM STEELS

IT appears from the series of tests that the Bureau of Standards has carried out upon the corrosion of chromium steel that the behavior of the material when subjected to the acid test is not a sure criterion of its resistance to atmospheric corrosion. Of all the alloys examined, a high nickel-chromium steel, invar, pure iron and medium carbon-steel that cooled very slowly from a high temperature were the most resistant to hydrochloric acid as measured by the loss of weight per unit area per day. High-chromium steels, for example, 13.70 per cent of Cr, 0.29 per cent of C, were found to be attacked by acid very much more readily. However, when the same specimens were subjected to a weathering test, consisting of a partial immersion in water and exposure to the air, the order of resistance was almost completely reversed. The high-chromium steels were the ones to withstand the treatment best, the low-chromium ones and the pure iron showing rust spots early in the test. The combination of both nickel and chromium appears to make the steel resistant to both acid and weather attack. In general, the steels that were quenched were found to resist corrosion better than the same material in the annealed state, but the differences found were much less than the differences resulting from composition changes, thus indicating that composition rather than treatment should receive the first consideration.

OXYGEN IN IRON

A SERIES of melts of electrolytic iron have been made at the Bureau of Standards under air in the induction furnace with additions of varying amounts of metallic calcium as a deoxidizing agent. The addition of calcium up to 0.2 per cent did not increase the soundness of the chilled cast ingots. The amount of oxygen, as determined by the Ledebur method, in the ingots was practically the same throughout the series, regardless of the amounts of calcium added. The average oxygen-content of the calcium-treated melts was approximately 0.23 per cent. Pure electrolytic iron melted under the same conditions contained 0.21 per cent of oxygen. All the above-mentioned melts were made in the open under still air. Electrolytic iron melted under a jet of air contained only slightly more oxygen than when melted under quiet air, namely, 0.24 per cent. Ingot iron melted under similar conditions under quiet air and under an air jet contained, respectively, 0.21 and 0.26 per cent of oxygen. Ingot iron melted under the oxidizing flame of a gas blast-torch contained 0.26 per cent of oxygen, the same as when melted under a jet of air.

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Economics of Motor Transport

By MERRILL C. HORINE[1]

ANNUAL MEETING PAPER *Illustrated with* PHOTOGRAPHS AND DIAGRAM

THE author states that motor transport today is threatened with arrested progress due to the lack of economic coordination between motor-vehicle operation, highway construction and legislative regulation. Highways constructed at considerable cost to the public have gone to pieces everywhere, in many cases years before their bond issues have matured. Efforts to preserve these roads have been confined principally to heavy taxation and restriction of motor transport; they have not been made upon a sound economic basis, largely because principles of highway-transport economics are not only imperfectly understood, but have hardly been studied sufficiently to provide any definite basis of understanding. The development of highway construction and motor-truck operation must proceed along sound lines to protect the public against serious and unnecessary losses, to preserve what highways we have, and to build a foundation for better ones in the future and to forestall disaster to the builders and the thousands of users of the most economical types of highway-transport vehicle. The solution of this matter is the application of scientific inquiry and engineering skill to the economic problems presented. As a contribution to preliminary thought on the subject, this paper was prepared for the purpose, principally, of stating the main features of the problems involved in their interrelation and significance.

An effort has been made to represent graphically the factors entering into the ideal of motor-transport economics; namely, economic efficiency. The conclusions hazarded in the paper are suggestive rather than final in many cases and no attempt has been made to evaluate definitely the various factors of efficient motor transport. It is hoped that the paper will provoke thought and discussion among engineers, to the end that the considerable volume of research necessary to arrive at such evaluation can be promoted.

IT would be absurd to state that the engineering development of motor-truck and highway-tractor design is at an end or that, even in the face of the greatest economic emergency, it should be halted; but the time has come when the engineer must turn his scientific inquiry and deduction into other and equally important channels. The development of the motor truck has placed it in a position of economic and industrial importance that demands the application of engineering to the broader aspects of motor transport. The highway, which until the development of the railroad was the chief factor in the economic and military progress of nations, is once more taking its place as the most important factor in transportation. First the war, then the railroad demoralization that followed it and lately the threatened railroad strike, demonstrated the emergency value of motor transport in industrial life, and in commerce.

It is not generally known or even suspected by the great mass of people that the motor truck is today transporting approximately one-eighth of the tonnage transported annually by all of the railroads in this Country. Motor trucks transport approximately 300,000,000 tons as against about 2,400,000,000 tons transported on rails. Yet, of the 2,000,000 miles of highway in this Country, only about 12 per cent or 300,000 miles is improved. Motor-truck operation is confined principally to this 300,000 miles, which is 20 per cent more than the 250,000 miles of railroad in the United States. Authorities have been predicting for years that, despite the tremendous size and importance of the passenger-automobile industry, the motor-truck business is destined before long to exceed it in size.

The development of the motor truck has been so rapid that highway development has by no means kept pace with it; in fact, highway development lags far behind the requirements for passenger automobiles. It can be said truthfully that modern types of road pavement are the outgrowth of motor-car development, but there is evidence on every hand that the majority of so-called motor-vehicle roads are not adapted to motor trucks.

Until recently, what little scientific research there was in respect to motor transport centered about the economy of the vehicle itself, while that devoted to highways took little or no account of the most economical types of motor truck. Such narrow conceptions inevitably lead to uneconomic conclusions. The student of vehicle efficiency alone soon finds that the larger the vehicle is and, consequently, the greater the useful load that it moves, the more economical in cost per unit of load it will be. The highway engineer, on the contrary, reaches the conclusion that the most economical road is one of light construction, narrow width and general conformity to the topography of the country. The vehicle economist therefore urges trucks of the greatest capacity that can be operated conveniently and for which there is sufficient load to carry, and the highway engineer urges nothing but the lighest of vehicles. The result is a compromise which is neither scientific nor economic, since, as a rule, the actual roads are of insufficient strength and durability to withstand the exactions of prevailing traffic, and the average vehicle is too small to produce the haulage economy which is needed. Vast sums are squandered annually on types of road utterly incapable of returning in traffic facility and durability enough value to justify their cost, and the public is obliged to pay, in the cost of commodities, an unduly high transportation-cost because of bad roads, poorly adapted routes and unreasonable restrictions upon weight and speed.

Coordination between highway and vehicle development is difficult because of the independent control of vehicle operation and of road building, but it is impeded still further by the lack of understanding that exists as between the highway authorities and motor-truck operators, and by the confusion of issues by the general public. The problem of the railroads has been very much simpler, since those charged with maintenance of way and those responsible for the operation of rolling stock move under the control of a single directing management, the engineering activity of all branches of the enterprise being consolidated and coordinated. The result has been the development of locomotives and cars capable

[1] M.S.A.E.—Engineer, International Motor Co., New York City.

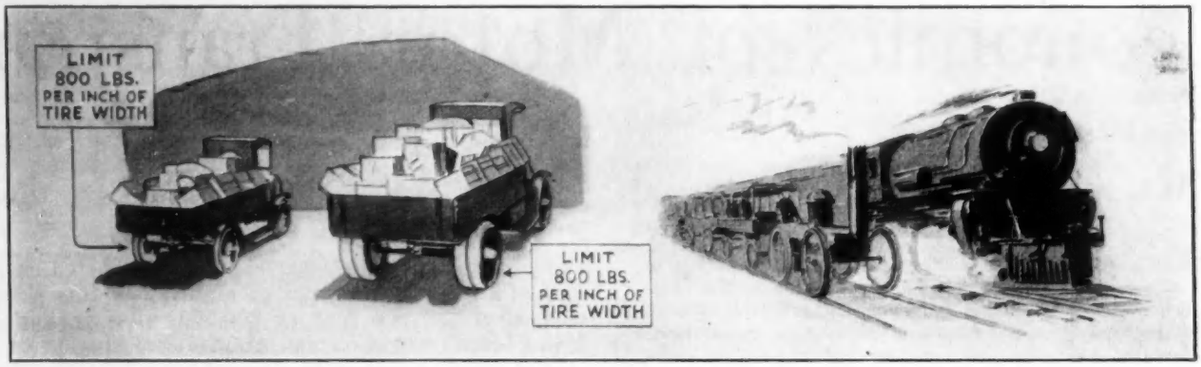

FIG. 1—HIGHWAYS MUST BE DEVELOPED IN CONJUNCTION WITH VEHICLE PROGRESS

Railroad Development Was Not Retarded by the Existence of Iron Strap Rails for Such Trains as Were Drawn by the DeWitt Clinton, but Has Kept Pace with the Increase in the Weight and the Speed of the Rolling Stock

of moving 5000 tons in one traffic unit, and of roadbeds and rails capable of supporting such loads at economical speeds. The engineering work devoted to the development of the railroad grade, track, bridges and other works included in the right-of-way has required a degree of intensive study not inferior to that directed toward the perfection of the rolling stock.

Coincident with this work, economic engineering of the highest order has been necessary to coordinate the development of the two so that the result would be conducive to the most economical transportation. Independent work by either the civil or the mechanical branches of railroad engineering could never have reached the point that they have attained in conjunction, as indicated in Fig. 1. Likewise, highway and vehicle engineers can never develop highway transport to its ultimate efficiency without coordinating their activities.

Legislation enacted independently by the 48 little republics that constitute this Nation cannot do otherwise than increase the economic loss resulting from the mutual unadaptability of the motor truck and the highway, as indicated in Fig. 2. The railroads were the target for restrictive attention by legislators not so very long ago, and it is admitted universally today that legislative meddling with the railroads has not been conducive to increased efficiency, lower charges to the shipper or industrial peace. Hundreds of official inquiries into the cost of living have been unavailing, and the economist's voice in the wilderness pointing to the deep underlying causes is still unheard.

The success of the automotive industry depends upon the degree of utility of its products to the public. Anything that detracts from the utility of the motor truck threatens the welfare of every member of the automotive industry. There is nothing that has so profound an effect upon prosperity as transportation. All industry exists as a result of an unequal distribution of commodities geographically, and consists of the production of a surplus where such production can be carried on most economically to supply a deficiency at some other location. The history of any manufactured article is a history of a long journey; in many and perhaps in most cases the cost of development represents the greater part of its cost.

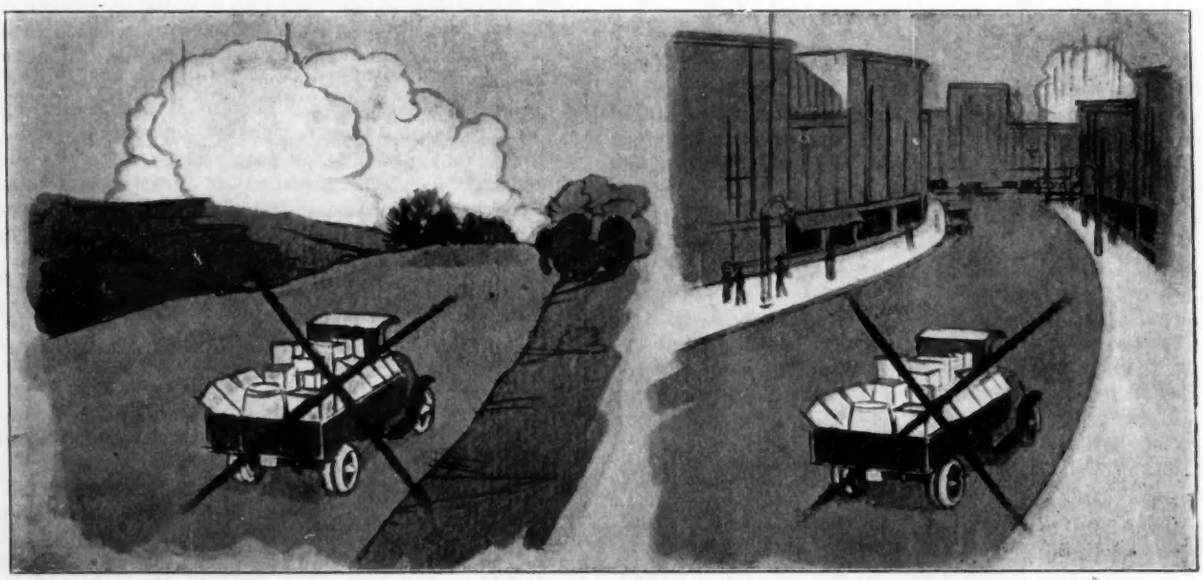

FIG. 2—INDISCRIMINATELY LEGISLATING THE MOST ECONOMICAL CARRIERS, THE BIG TRUCKS, OFF THE HIGHWAYS, CITY STREETS AND COUNTRY ROADS IS AN ECONOMIC WASTE; HIGHWAYS SHOULD BE BUILT TO CARRY THE BIG TRUCKS

While the Cause of This Action Is the Inability of Some of the Poorer Roads To Withstand Modern Traffic, Such a Practice Increases the Cost of Transportation and the Latent Value of Good Roads Is Not Realized

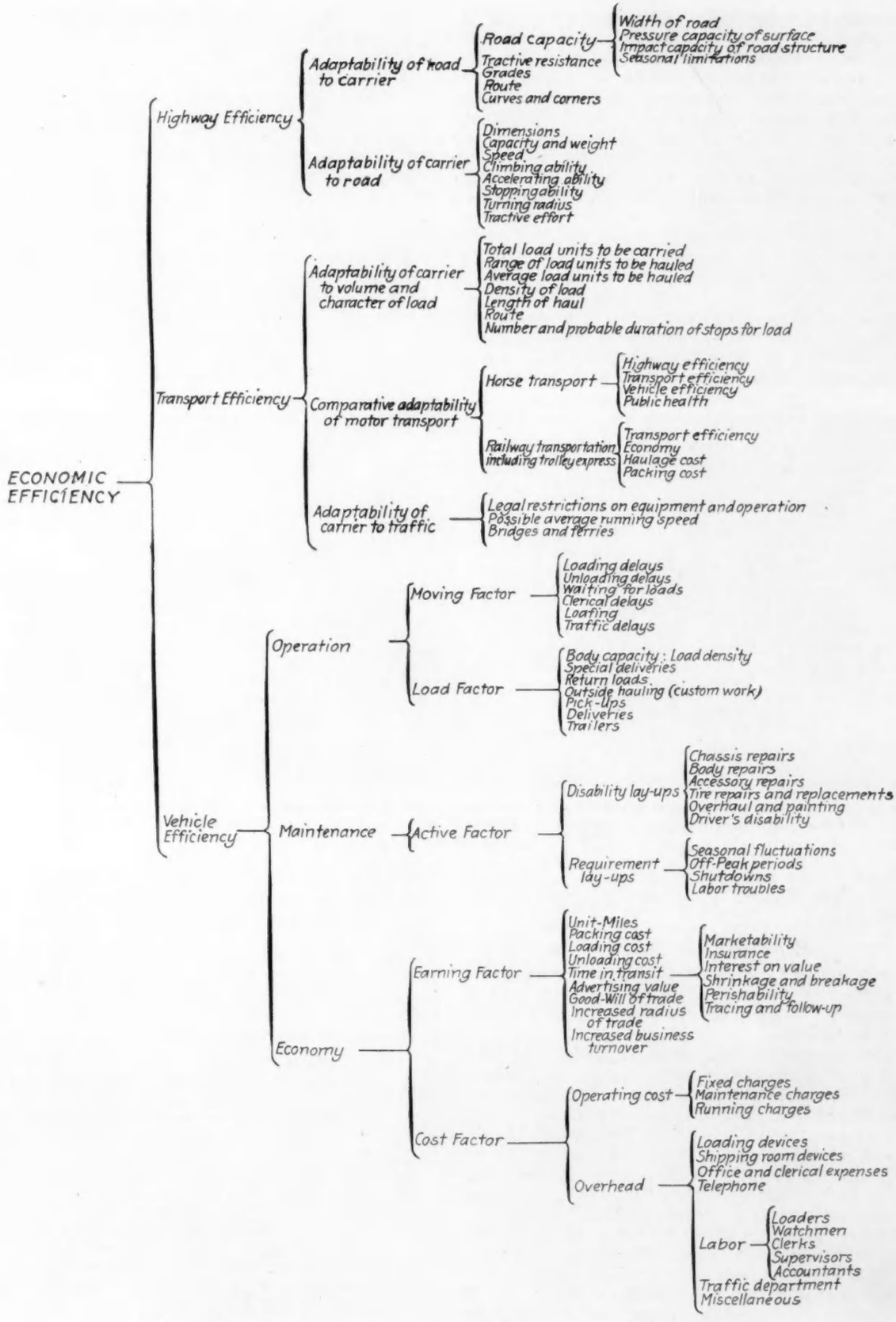

FIG. 3—CHART SHOWING THE ECONOMIC EFFICIENCY FACTORS OF MOTOR TRANSPORT

Motor-transport economics, therefore, represents the means whereby the public can be enabled to derive more benefit from the product of the automotive industry, and involves the scientific study of all factors relating to the transportation of loads over roads. The study of motor-transport economics calls for engineering, both civil and mechanical, the principles of which are common and combine considerations of mechanical efficiency, road structure and commercial traffic. It contemplates highway transport in relation to the interests of the public as a whole.

Fig. 3 is an outline of the principal elements of the subject in their intricate relation. Each element is combined with related elements to form a single factor. These factors, in combination with other related factors, are reduced to terms whereby a balance can be secured between antagonistic considerations to make up a coherent conception of their interrelation. For lack of precedent, arbitrary terms have been used to designate the various factors and groups of factors. The entire subject has been reduced to a single term, that of "economic efficiency."

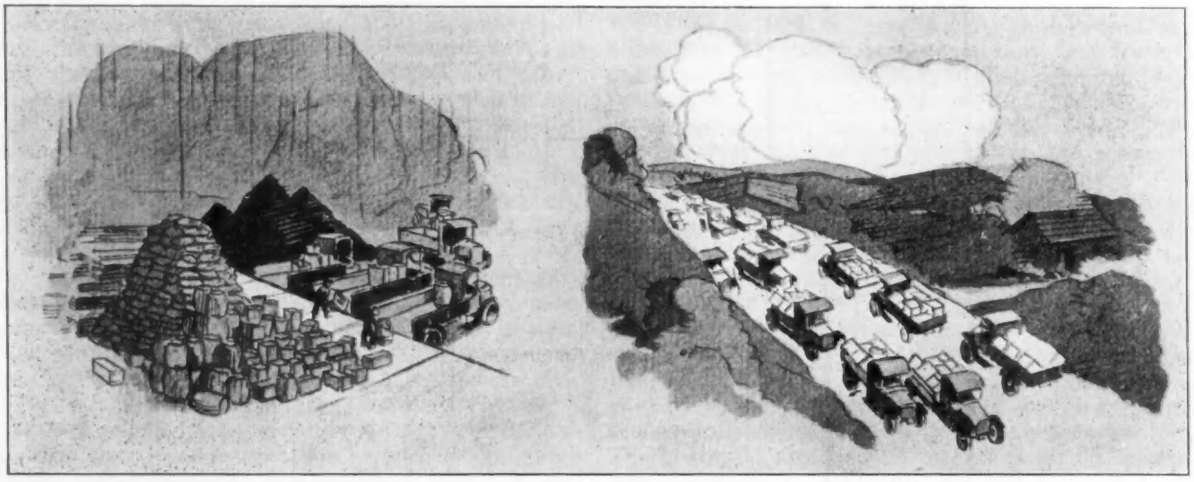

FIG. 4—SCRAPPING THE BIG TRUCK WILL NOT DECREASE THE AMOUNT OF GOODS TO BE MOVED
This May Increase the Burden on the Community Instead of Effecting a Saving, Since a Larger Number of Smaller and Less Economical Trucks Must Be Employed

ECONOMIC EFFICIENCY

Economic efficiency, as related to highway transport, can be defined as the measure of net benefit derived by the community arising from the operation of a motor-transport installation.

We of the automotive industry must get into the habit of thinking of motor-transport economy in the terms of cost to the consumer. We cannot hope to have the strong, hard, smooth, wide highways, connecting industrial centers by direct routes, which we know are essential to the proper and continued expansion of the motor-truck industry, before the public shall have been convinced that the cost of such roads is merely an investment in cheap transportation that will be returned with interest in the form of lower commodity-prices by reason thereof. The highway industries must likewise revise their conception of the motor vehicle, and particularly of the motor truck, from that of a trespasser on the roads to that of an essential and economical carrier capable of earning its share of the cost of roads capable of accommodating it. They must be brought to realize that the highway exists for the benefit of the vehicle, that its usefulness consists of its ability to serve the vehicle, and that its economic justification is the saving in transportation cost that it enables the vehicle to effect.

Finally, the general public must realize its position as a stockholder in every transportation enterprise. Public investment in highways pays dividends in the form of lower transport-costs than would prevail without them, and in proportion to the extent to which highways and vehicles are adapted, on the one hand, and the extent to which this adaptability permits the operation of the most economic types and sizes of motor truck and methods of operation, on the other hand. Taxation of motor trucks out of all proportion to the benefits derived from their operation does not shift the burden from the commonwealth to the private pocket of the truck owner, but simply requires that highway cost be paid in the form of increased cost of commodities instead of in taxes, with the constant danger that the continual readjustment of charges for haulage and commodities hauled by motor-truck users will result in waste and commercial losses, including the retardation of motor-transport development; all of which reacts unfavorably on the public purse.

Restriction of sizes and weights of truck for the purpose of prolonging the life of uneconomic types of road, such as to prevent the operation of the most economical type and size of truck, will prove expensive to the public in the end for two principal reasons

(1) The elimination of the heavy truck will not reduce the volume of transportation over the road, but will divert it to smaller vehicles in greater number; this will occasion a considerably higher transportation-cost, as is illustrated in Fig. 4

(2) The operation of this greater number of smaller vehicles at higher speeds and generally greater impact upon the road will result in more rapid road-wear and will require, in time, the widening of the road to accommodate the large number of vehicles. The cost of a wider road of lighter construction may very possibly be greater than that of a narrower one of heavier construction, as is shown in Fig. 5

The maximum economic efficiency, therefore, depends upon the adaptability of the road to a type of vehicle that is the most adaptable to the nature of the haulage work

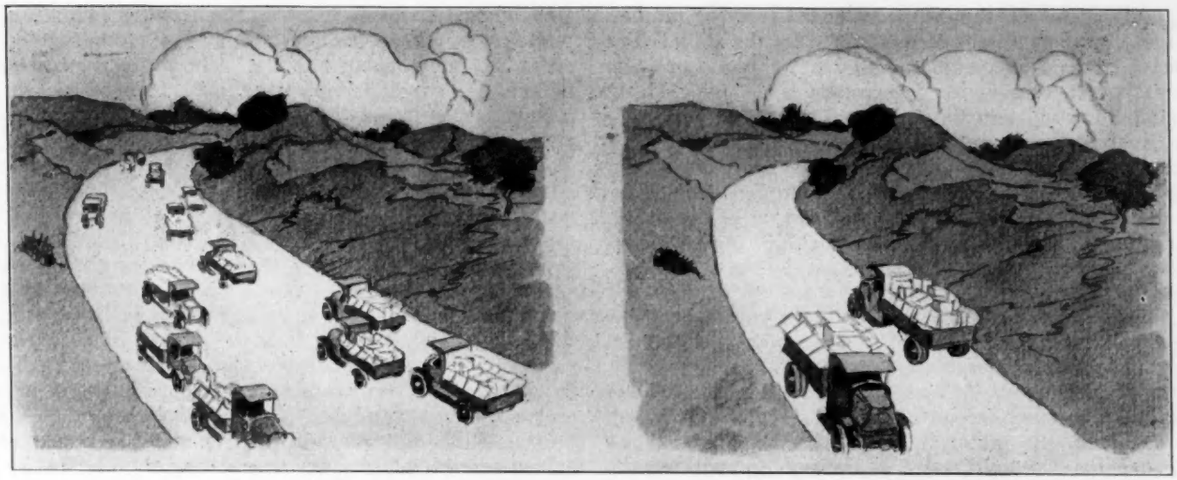

FIG. 5—LIGHT-TYPE ROADS ARE NOT NECESSARILY MORE ECONOMICAL TO BUILD THAN STRONG ONES
Restricting the Traffic to Small Vehicles Means Multiplying Their Number and Hence a Greater Width of Road Is Required for Their Accommodation

to be performed, operated in the most effective and economical manner. These considerations involve three groups of factors which can be termed highway efficiency, transport efficiency and vehicle efficiency.

HIGHWAY EFFICIENCY

Highway efficiency consists of (*a*), the adaptability of the road to the carrier and (*b*), the adaptability of the carrier to the road. In other words, a highway may be efficient by reason of its appropriateness to prevailing types of vehicle or because the vehicles are adapted to its particular construction, as is indicated in Fig. 6.

As shown in Fig. 3, the adaptability of the road to the carrier requires that it possess the proper capacity, tractive resistance, grades, routes, curves and corners; in other words, it must be wide enough to permit the traffic to flow freely and without unnecessary delay. What width will best accomplish this end will depend naturally upon the types of vehicle making up the traffic and the volume and speed of such traffic. It must have a surface capable of withstanding the pressure of the tires and a structure capable of withstanding the impact occasioned by the passage of the vehicles. It must be of such nature that the changes of condition with the seasons or weather will not interfere with its use. It must have a surface that produces a low tractive-resistance, grades that are within the practical ability of the truck and a route calculated to give direct and easy access to the points that the truck must reach. Curves and corners must be such as to permit the vehicle to negotiate turns without difficulty. The adaptability of the carrier to the road naturally depends upon characteristics of ability complementary to the above-specified elements.

A road that is narrow, soft and poorly drained, having steep grades and sharp curves, is naturally very poorly adapted to heavy tractors and trailers, the characteristics of which require a road of at least 16-ft. width having a smooth, hard surface, a firm subgrade, good drainage, moderate grades and gradual curves wider than the straight portions of the road.

TRANSPORT EFFICIENCY

Transport efficiency consists of (*a*), the adaptability of the carrier to the volume and character of the load, (*b*) the comparative adaptability of motor transport to horse transport or railroad transportation and (*c*), the adaptability of carrier to traffic conditions.

Again, as shown in Fig. 3, the adaptability of the carrier to the volume and character of load depends upon the total load-units to be carried, the range as constituted by the maximum and minimum load-units to be hauled per unit of time, the average load-units per unit of time, the density of the load, the length of haul and its route and the number and probable duration of stops for load.

By the term load-unit is meant the unit by which the load is measured; such as tons for coal, cubic yards for

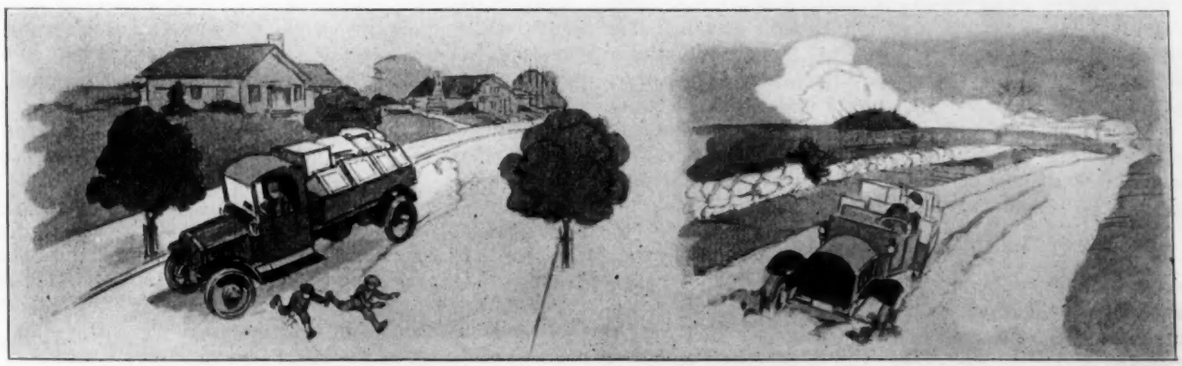

FIG. 6—THE RESTRICTION OF TRUCK SIZE WITHOUT ANY RESTRICTION ON THE LOAD WILL NOT ADEQUATELY PROTECT ROADS THAT ARE NOT STRONG ENOUGH TO MEET THE TRAFFIC REQUIREMENTS
The Restriction of Truck Size Encourages Overloading. Overloaded Light Trucks May Be More Destructive Than Properly Loaded Heavier Vehicles and Are Also Dangerous

sand, gallons for oil, thousands of board-feet for lumber, packages for department stores and cans for milk. The character of load-unit and the kind of material or commodity that it represents determines the type of body best adapted to efficient haulage; and the maximum, minimum, average and total amounts to be hauled, together with the length of haul, determine the most economical size of chassis. The route and the number and probable duration of stops for load have a considerable bearing upon the type of equipment that is most economical; that is, whether a truck, a tractor with semi-trailer, a truck with full trailer or a tractor and trailer train is most suitable; whether solid or pneumatic-tire equipment should be provided; and whether gasoline or electric power should be used.

If the work involves a long haul over a route including soft and tortuous mountain-roads, with few stops of short duration, the material being in units of packages of low density, the equipment is limited immediately to a light-weight, high-speed pneumatic-tired chassis having an enclosed or a screen-sided panel-body equipped with means for readily taking out individual deliveries. The equipment will naturally have to be gasoline-propelled, preferably provided with electric light but not necessarily with electric starting. If, on the other hand, the work involves the haulage of a large quantity of an exceedingly dense material of a non-fragile nature, in cubic-yard units over only a few miles of reasonably hard and level road, with no stops for loads between terminals and subject to considerable variation in the number of cubic yards to be transported per day, the most economical equipment is a 7½-ton chassis having a wide, flat, steel, mechanical-dumping body, with full trailers having similar bodies for use when the volume of work is particularly heavy.

The comparative adaptability of motor transport to other means of transportation depends upon the comparative adaptability of different possible methods along the line already stated. In the consideration of horse transport, practically every factor concerned with motor transport must be taken into account. In addition, the factor of public health will become more important. In connection with railroad transport, the comparison with motor transport is limited to consideration of transport efficiency and economy. Moreover, railroad transportation must be judged also from the standpoints of the cost, delay and breakage incident to carriage over the streets at each end of the rail haul, and the extra packing, boxing and carting required in railroad transportation. In most cases it is not difficult to define the separate fields of efficient transport of the horse, the railroad and the motor carrier. None of the three is capable of serving for all kinds of transportation efficiently. The field of motor transport lies in a ground between economical horse and railroad transportation. The adaptability of the carrier to traffic conditions depends upon how it conforms with State and local regulations, the average running speeds that are possible in prevailing traffic and the degree to which it is adapted to bridges and ferries on the route, as illustrated in Fig. 7.

Vehicle Efficiency

Vehicle efficiency is concerned with the comparative amount of benefit derived from the operation of the equipment for the amount of expense of one sort or another incurred by its operation, and can be expressed as the combination of effective operation, efficient maintenance and economy of cost.

Effective operation can be expressed as the combination of two factors; namely, the moving factor and the load factor. The former is simply the percentage of time spent by the truck in moving. The latter represents the percentage of load capacity that is constituted by the actual average load carried. Fig. 3 gives a few of the principal elements of operation that affect these factors. Thus, a truck can be considered to have a 100-per cent operating efficiency when the wheels are turning every minute of the working day and it is carrying a full-capacity load at all times. Such a condition could rarely exist, but it is conceivable that a truck might be used in spreading road material in such a manner that it would receive its load from a traveling chute without stopping at the loading point, and would spread it on a road or yard surface while moving through the unloading point. Ordinarily, a moving factor of 60, representing 6 hr. running out of a 10-hr. day, and a load factor of 50, representing a 2½-ton average load on a 5-ton truck, are to be considered satisfactory operation.

Maintenance efficiency is expressed by a single factor; namely, the active factor, by which is meant the percentage of working days during which the truck is in commission. An active factor of 100 per cent represents a truck in commission 310 working days in a 310-day year. Fig. 3 indicates two classes of lay-up that commonly are responsible for an active factor of less than 100 per cent; namely, those due to disability of the truck and those due to conditions in the owner's business that render the operation of the truck unnecessary. An active factor of 100 per cent is by no means uncommon among the better grades of chassis.

Economy, in the narrower sense in which it is used in connection with Fig. 3, consists simply of the ratio between cost and earning. These are termed, respectively, the earning factor and the cost factor. Included in the earnings of a motor truck are all the benefits that accrue to the user by reason of its operation, including the savings effected over what his costs would have been by the next most suitable form of transport. Fig. 3 only outlines and suggests a few of these benefits, which must of necessity be of limitless variety in different installations. The cost factor is made up of two groups of expense; namely, the operating cost and the overhead. Included in the operating cost are the usual fixed, maintenance and running charges, covered by the National Standard Truck Cost System, and under overhead are all other charges concerned with the business that arise from or are necessitated by the operation of motor transportation.

It is possible to derive some measure of the economic efficiency of a motor-transport installation out of a combination of all of these factors. These are the materials with which we have to work, the remaining problem in arriving at a conclusion being to evaluate and harmonize them in such a manner as to attain an equation in which all factors are in their proper proportion. To do this, it is first necessary by experiment, research and calculation to base as many of these factors as possible upon definite determination of facts, many of which are exceedingly obscure today. As might be expected, it is not possible within the limited compass of a single paper to more than reconnoiter the ground to be covered by a research development that it is hoped will enlist the co-operation of automotive and highway engineers and economists throughout the industry.

Lines of Necessary Research

Some of the principal subjects for needed research for the development of what can be termed the economics of

FIG. 7—LARGE-CAPACITY TRUCKS ARE BETTER ADAPTED TO CITY TRAFFIC CONDITIONS THAN SMALLER VEHICLES
The Obligatory Slow Speed Is Normal to the Large Truck and It Can Deliver Large Daily Tonnages Despite Traffic Conditions, and Since the Same Amount of Tonnage Can Be Transported in Fewer Vehicles There Is a Tendency Toward a Reduction in Traffic Congestion

motor transport are (*a*) highway engineering, (*b*) highway economics, (*c*) traffic investigation and (*d*) automotive engineering. In regard to highway engineering, they are

(1) Safe tire-loads for various types of road construction
(2) Safe total axle-weights for various types of road construction
(3) Impact resistance of various types of road construction
(4) Tractive resistance of various types of road
(5) Super-elevation of curves
(6) Relation of necessary widening of road at corners to degree of curvature
(7) Types of bridge appropriate to various types of road
(8) Necessary degree of surface smoothness to reduce impact to harmlessness

Under highway economics are included the

(1) Construction and maintenance cost per mile of various types of road, pro-rated over the life of the road
(2) Vehicle capacity of road per hour, under practical traffic conditions
(3) Tonnage capacity of road, with various types of traffic, in reference to the speed and capacity of trucks
(4) Proportion of highway and vehicle cost respectively to the total cost of motor transport

In traffic investigation, the need is for a traffic census on important street and highway motor-truck routes. Such an investigation should show not only the total number and types of vehicle traversing the road, but the load factor and speed under which they are operated and changes in volume at different periods of the day, all such data being collected at various times and at several points so that the following factors can be determined

(1) Daily tonnage hauled over the route
(2) Daily vehicle-mileage over the route
(3) Average load per vehicle
(4) Average miles traveled on the route per vehicle
(5) Ton-miles per day on the route
(6) Comparative ton-miles at different hours of the day and on different days of the week
(7) Comparative ton-mileage at different times of the year
(8) Comparative ton-mileage at different points of the route
(9) A study of comparative costs of transportation by railway freight, express and trolley express

Such a study should contemplate the complete cost for different classes of material commonly hauled on motor trucks, including freight charges, trucking at the point of origin and destination, and extra packing-costs.

Automotive-engineering research is desirable on the effect of grades, relative smoothness of surface and tractive resistance upon

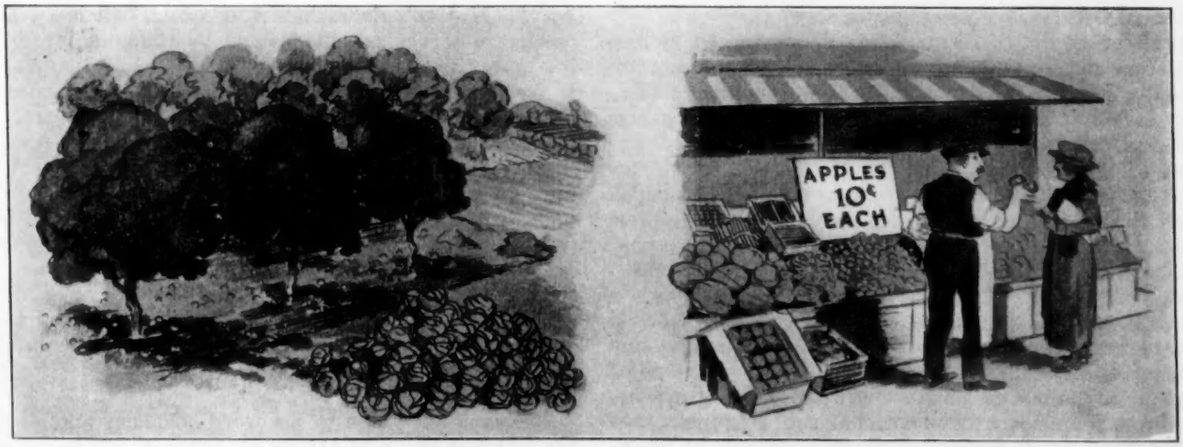

FIG. 8—MOTOR TRUCKS MAKE POSSIBLE QUICK AND CHEAP MARKETING OF MANY COMMODITIES
Without Motor Trucks Crops, Milk, Livestock and Other Commodities Either Would Decay, Sour or Shrink in Transit or Else Necessitate Higher Retail Prices Resulting from Their Scarcity

(1) Gasoline consumption
(2) Speed
(3) Drawbar-pull
(4) Acceleration

Work along these lines is in progress. The Bureau of Public Roads of the Department of Agriculture is spending $150,000 per year on road research and experiment. The various States and the National Research Council also are vigorously prosecuting work along similar lines. The Iowa Engineering Experiment Station, for example, has $10,000 per year to spend upon highway research, of which it is understood about $7,500 will be devoted to an economic study of highways.

The National Research Council, through its Committee on Economic Theory of Highway Improvements, under the chairmanship of Prof. T. R. Agg, is conducting a series of tests at the Massachusetts Institute of Technology to determine some of the physical relationships between the vehicle and the highway.

Experiments begun by the Office of Public Roads, under the direction of A. T. Goldbeck, are being continued. Mr. Goldbeck, as chairman of the National Research Council's Committee on Structural Design of Roads, is making a new set of tests on the Illinois experimental highway.

The $275,000,000 Federal aid appropriation for highways has not been spent by any means. It is estimated that the annual highway expenditure of the Federal Government, the several States, and the counties and townships within those States, amounts to $1,000,000,000 per year. As against this, motor-vehicle owners contribute the largest single item, conservatively estimated at $316,720,878, of which $102,000,000 is in registration and license fees alone; and the amount being contributed by motor-vehicle users in the form of direct taxation is increasing rapidly.

There can be no doubt that the principle of taxing the users of the highway for the privilege of making use of the public roads is faulty, especially when the basis upon which the tax is levied is unscientific and inequitable, applying only to power-driven vehicles in most cases, while permitting animal-drawn vehicles free use of the highway. Unquestionably, the benefits of the highway extend to every citizen, irrespective of whether he operates a motor vehicle. This special taxation is inevitably passed along to the general public in the form of increased price of commodities, illustrated in Fig. 8. The practice is gaining impetus throughout the States. Texas has recently passed a law whereby motor trucks are taxed a certain amount per mile operated.

Acquiescence to this condition of affairs by motor-vehicle users carries with it the right to demand a voice in highway councils to the end that the money shall be spent on types of highway construction that are economically suited to the transportation needs of the Country. It is the engineers of this automotive industry who must lead in furthering the proper economic development of the highways and the development of motor-transport.

THE DISCUSSION

CHAIRMAN B. B. BACHMAN:—Mr. Horine has touched upon a very fertile subject for discussion. The question of highways in all its ramifications is one of the most important that the automotive engineer of today has before him. It is not a problem that can be compressed within narrow limits, and it has an uncanny faculty of intruding itself in the most unexpected manner. The need of the Country for roads is unquestioned. The possibilities in earning power of completed roads adequate for the transportation of the commodities of the Country, without infringing in any way upon the functions of the public-utility companies, are enormous. Whether the figures that Mr. Horine has given are correct or not, within even a relatively large percentage, appears to me to be comparatively unimportant. The figures are tremendous; but almost any figures may be assigned and still not touch the possibilities or describe the actualities.

There is no doubt that we are face to face with a very practical problem. How shall we finance the construction of these roads? How can we sell this idea to the public? As Mr. Horine has indicated, there are a number of serious errors crystallizing in the public mind. I am rather fearful that we of the automotive industry, as a class, have been somewhat to blame for these erroneous beliefs. I am somewhat apprehensive lest the possibilities of commercial advantage may have blinded us to the future. This is not only true in reference to the truck and its relation to the public, but with regard to traffic delays. It is not pleasant to drive down a street and be in one of the 20 or 30 vehicles behind a trolley-car that stops at a crossing. On the other hand, the pedestrian has as much right on a highway as the man in the automobile and, in most cases, he is considerably at a disadvantage. Congestion and delay are necessary to protect that man's rights. They are an economic waste, there is no question about that, but they must be overcome by constructive criticism and reasonable suggestion.

The paper Mr. Horine has given presents an excellent point of view. It is possible that there are some who differ from his opinions, but the function of these meetings is to bring out these differences and, by doing so, to attempt to formulate a policy that will overcome and correct the difficulties. This is the fundamental reason for our meeting.

R. E. PLIMPTON:—Mr. Horine has really discussed the scientific utilization of motor transport, rather than its economics. He says as much in the statement that the subject "involves the scientific study of all factors relating to the transportation of loads over roads." I make this point because it seems to me that the term economics had better be used in its generally accepted meaning, as relating to the financial aspects of the subject.

The use of the term efficiency is also to be questioned. It seems to refer to almost any of the divisions under which Mr. Horine considers the subject of motor transportation. This may seem to be mere quibbling about the meaning of words, but that this is not true is shown by Mr. Horine's definition of economic efficiency as the "measure of net benefit derived by the community arising from the operation of a motor-transport installation." It would seem preferable, particularly in a discussion before a body of engineers, to use this term as meaning the ratio of the total cost of operating a motor-transport system to the total revenue derived from it. Then we have an efficiency or ratio measuring performance, based upon economic units of dollars and cents. This efficiency, as thus defined, will become of greater importance as the art develops. The cost of operation will be known more accurately, and a comparison can be made even if it is not possible to calculate actual revenue, by using the income derived from motor-haulage by companies, railroads or other organizations making a business of furnishing transportation.

The same objection to the word efficiency applies to its use in other parts of the paper but not, of course, when it refers to a definite numerical ratio, as in Mr. Horine's discussion of effective operation and of maintenance effi-

ciency, better called effective maintenance. The explanation of the latter, however, states that it is expressed by a single factor, the percentage of working days the truck is actually in commission. This does not measure completely the effectiveness of the maintenance. Why not combine two factors, the percentage as proposed by Mr. Horine and one measuring the effectiveness of fuel consumption? The latter could be expressed as the ratio of fuel actually used during the working year to the amount that would be used were the truck always in the best operating condition.

The carrying of "live" freight has not been considered in the paper, although such service is one of the most important which now makes use of automotive equipment. It has been well said that passengers are only freight that is self-sorting and self-handling. The motorbus of the present, or at least the type now used by responsible companies in regular service, is built ordinarily on a motor-truck chassis; consequently, its economics and utilization are properly to be considered in any discussion of motor transport. The motorbus of the future probably will be a special type of truck, with provision for higher speed, better riding qualities and lower floor-level than the vehicles designed for freight service. G. A. Green, of the Fifth Avenue Coach Co., said in his paper on Motorbus Transportation[1] that the motorbus should be a development of the passenger-car chassis.

A vehicle carrying both freight and passengers is likely to be in demand in sparsely settled districts. Table 1 is presented to show how far this movement has developed in California, where the roads and climate are good and the communities are scattered widely. The data in the table are based upon reports from 773 companies authorized to do business in California.[2]

TABLE 1—ANALYSIS OF BUS OPERATION IN CALIFORNIA

	Companies	Per Cent
Passenger	425	55.0
Freight	117	22.8
Passenger and Freight	97	12.4
Passenger and Express	11	1.4
Freight and Express	7	0.9
Passenger, Express and Freight	6	0.9
Freight, Milk	18	2.3
Freight, Milk by Contract	1	0.1
Parcel Delivery and Passengers	1	0.1
Freight, by Contract	6	0.9
Special Freight	6	0.9
No Data Given	18	2.3
Total	773	100.0

Summarized, Table 1 shows that 55 per cent of these companies are carrying passengers and 14.8 per cent passengers and freight; 27.9 per cent carry nothing but freight. This is not the place to discuss the subject in detail, but it may be said that the motorbus is already setting the pace for the scientific utilization of motor transport. There are several reasons for this. Motorbus operators make a business of transportation and in many States they are required to submit complete and accurate costs of operation. Effective operation is essential if they are to conduct their business at even a fair profit.

Mr. Horine's paper forcefully reveals the fact that we are approaching the third phase of the automotive industry. The first phase, with its construction problems, and the second phase, with its development of selling methods and channels, are now accompanied by a third phase in which the problems of utilization are being studied thoroughly.

M. C. HORINE:—Concerning Mr. Plimpton's criticism of the term "economic efficiency," I supposed that it would be understood to mean the economic efficiency of highway transport and not interpreted broadly. Mr. Plimpton objects to that term as being broader than the definition, and proposes that we should confine it more to matters of cost and income. This seems to me to narrow it. I thought I made it broad enough when I said that it was a measure of net benefit derived by the community arising from the operation of motor transport. This includes not only cost, but many other broader considerations.

I am sorry Mr. Plimpton and I do not define efficiency alike. I have called it the "measure of net benefit"; Mr. Plimpton prefers: "the ratio of the total cost of operating a motor-transport installation to the total revenue derived." Referring to the chart in Fig. 3, we find that what Mr. Plimpton has defined is vehicle efficiency and not economic efficiency. In either case, however, Mr. Plimpton and I evidently agree as to the meaning of the word, differing only in its definition, since the ratio of cost to income, as it refers to efficiency in a commercial sense, like the ratio of input to output, which is the accepted definition of mechanical efficiency, is certainly a measure of net benefit. Throughout the chart in Fig. 3 I have confined my use of the word efficiency to items involving ratios between income and outgo.

Mr. Plimpton misquotes the chart in Fig. 3 when he refers to "maintenance efficiency," for it refers to this item merely as "maintenance," although it might be called "effectiveness of maintenance." It is not an expression of efficiency, but must be combined with "operation" or operating effectiveness and "economy" to produce "vehicle efficiency." I confess I cannot follow Mr. Plimpton in his proposal for an additional factor under "maintenance" concerning gasoline mileage. Surely effective maintenance produces other benefits than gasoline economy; if the latter is given special consideration, such things as repairs, depreciation, tire economy and the like should be included also. That is the reason for using the factor "economy." Gasoline mileage is purely a matter of economy and, because the effectiveness of maintenance reacts upon economy, they are considered as coordinate factors of vehicle efficiency. Another reason is that, since maintenance is expensive, this expense must be justified in some manner other than low cost or effective operation, because the cheapest maintenance would be to lay the truck up altogether, occasioning no expense for repairs, overhaul or inspection. The effectiveness of maintenance is therefore measured by the "active factor," or ratio of days in commission to days when operation is possible.

Motorbus transportation has not been considered in this paper because I felt that the term "transport" refers more to freight transportation than to passenger traffic and because Walter Jackson had been scheduled to cover the subject at this session in a separate paper entitled The Past, Present and Future of the Motor-Omnibus.

A MEMBER:—What are the savings that might be effected with a truck over a good road as compared with the cost over a poor road? Consider a hard-surfaced road as compared with a macadam road that is badly cut up.

MR. HORINE:—That is a very important question. It is being investigated by the Massachusetts Institute of Technology. It is also under consideration by the Na-

[1] See THE JOURNAL, July, 1920, p. 67.
[2] See *Bus Transportation*, January, 1922, p. 27.

tional Research Council. Some very deep studies are being made along that line, but time is required to complete them. They are not yet complete. Various figures have been obtained on the resistance to traction of various types of pavement. Different authorities give a very great disparity of data on roads of the same description. That is the condition that prevents giving a categorical answer to the question at present. Undoubtedly, the great amount of research that is being done now along that line will develop some very definite figures.

A MEMBER:—Was this California experiment more of a road test than a mechanical truck test?

MR. HORINE:—The California test was entirely a test of a particular type of concrete road.

A MEMBER:—It seems to me that the question is one of relative efficiency of tires and surface. So far as the smooth surface is concerned, a hard smooth type will give the most efficient service. The history of the automobile industry, so far as passenger service is concerned, bears out the conclusion that a pneumatic tire gives the most efficient service. It gives a higher speed at less cost than any other type of tire ever put on wheels. So far as the point made by Mr. Horine is concerned, the impact of the pneumatic tire is much the same as that of the solid tire. It seems obvious to me that, where a cushion is created between the load and the surface over which it travels, one that is readily receptive of inequalities in the surface, this is, in effect, carrying the good road along with the vehicle, as has been argued for years in the pneumatic-tire industry. This is a fact and has been proved in thousands of instances in my own experience where, under what we call the rolling-resistance test, the same vehicle with the same load, when moved by gravity only, will register a difference in the amount of work performed as high as 100 per cent over other types of tire. Pneumatic tires are more efficient than solid tires. A cord tire will run from 40 to 60 per cent more efficiently in a purely rolling test than a canvas tire. The efficiency of a pneumatic tire is dependent upon the angle at which the tension members are laid with the tire, the air pressure that carries the load and the area of the tire that is brought into contact with the surface of the roads over which it travels.

It seems to me that urban and suburban passenger traffic ultimately could be moved on air just as well. We are only at the beginning of pneumatic-tire development. I believe firmly, after 30 years of experience, that we will be able to carry on air tires not only the maximum load of today, about 5000 lb. per wheel, but 10,000 lb. per wheel; and at a pressure not to exceed 50 to 60 lb. per sq. in. In fact, the limitation lies only in the size of the wheel that the automobile companies permit us to make. Therefore, I think my experience warrants me in saying that the future of the pneumatic tire for heavy duty is but beginning.

W. P. KENNEDY:—I believe that sometimes we gain an advantage by standing off and looking at our problems from a distance, rather than from our close position when we are engrossed with the details and are not able to discern all the possibilities of their solution. We can then assume a radical viewpoint. Mr. Horine's paper is primarily for the purpose of stimulating education in several directions, so that we can avoid the impediments or incumbrances that are likely to be imposed on the development of the automobile truck. There is evidently a vast need for education; in the direction of legislation, in the direction of motor-vehicle usage, and possibly in the direction of aiding the road-builders in solving their problems.

If we first consider the motor truck and ask ourselves what difference will exist between the present form and the future form in 25 years, a number of speculations arise. I agree that, in time, we will operate motor vehicles entirely on pneumatic tires. As we look at the truck now, there are two things that are fundamentally wrong with it; one is the mode of suspension and the other is the tire equipment, with reference to the work that must be performed by the truck. When a large truck is unloaded and operating in the street over a rough surface, we can imagine the energy that is being lost in raising the load over the obstructions of the street surface. We can get a fair vision of this when we ride behind a city taxicab of very light weight and notice that when the body is loaded it will remain practically in a horizontal plane and the axles will rise and fall with the impact of the road. If we could provide for motor trucks to function in that way, we would have an ideal condition. That condition probably can be approached by some form of progressive spring equipment that will take care of light as well as heavy loads. In a 5-ton truck operating empty, one might as well have cast-iron blocks in place of the springs, because the springs do not function at all; but if the truck had progressive spring equipment, the body would remain stationary with varying loads.

One of the steps that I think might be taken in the direction of equipping trucks ultimately with complete pneumatic equipment is to apply a dual equipment of a solid tire and a pneumatic tire side-by-side that would permit the solid tire to function when the pneumatic-tire load is exceeded. Progressive spring equipment could be furnished by combinations or auxiliaries that will be obvious to any good designer.

Looking again at our present procedure in the design of trucks, is it not reasonable to assume that there is considerable waste at present in that so many companies are building such varied lines of machines? Could not our present transportation requirements be fulfilled adequately by probably three sizes of motor vehicle? In the first size I would include those up to 1000-lb. capacity. Let the builders that are disposed to produce this line, largely from passenger-car equipment, construct the varied equipment that is at present on the market. The next is about a 2-ton size that would perform all the requirements that are necessary in cities. Let this 2-ton truck have an overload capacity up to 3 tons. For heavy work provide a truck that would nominally have a 5-ton capacity, but an overload capacity up to 6 or 7 tons with the necessary spring and axle modifications. The idea I have in mind is that, if the industry were concentrated upon these three sizes, great economics would be effected in the manufacture of parts, in the production of the machines, in the regulation of taxes and in the many other things that so complicate the question now, imposing as they do much waste and expense of which pioneers in the use of motor transportation must bear the burden. If it were thus possible to standardize upon the two larger sets of equipment, one in the 2-ton field and the other in the 5-ton field, and to allow those who are building light vehicles to include passenger chassis or anything else that they are disposed to use, much of the waste now existing in manufacturing tool equipment, selling effort and many other phases of the proposition would disappear.

With reference to educating business men in regard to the value of the motor truck, this frequently is difficult. The city users of the motor truck, as a whole, usually have been unaware of the cost of operation and, often, a user does not want to know or be worried with it. In

some cases he thinks he knows what his expense is but, when told that it costs more per year to operate a given size of machine than the truck originally cost him, he does not believe it. When he pays $5,000 for a truck and later learns that it costs him $6,000 per year to run it and that, in the 5-year life of the machine, its service costs him $30,000, he believes there is something wrong somewhere.

Methods of Road Construction

It was pointed out in the previous discussion that, if we could do anything to aid the Society in the general development of the industry, it would be welcome and proper for us to offer suggestions along that line. A thought or speculation about improved methods in the building of roads may be acceptable. It appears to me that there is considerable waste in the manner in which roads are built at present. First, it is a seasonable undertaking and there is only a limited time in the year during which work can be done. Roads are built in an ancient and crude manner; and, in so far as the handling of the material is concerned, there is practically no modern, scientific method in it.

Why would it not be possible to build our highways in the way we build our large office-buildings at present? The office building is built in a plant that has the tools and facilities for making the parts of the building most efficiently. Why would it not be a very desirable thing in the way of cost reduction to indicate to road-builders that at least the surface of the road might be built in a plant in relatively small unit-sections that would be convenient to transport? They could then have the proper mass-production equipment for doing it, they could work all the days of the year and the matter would resolve itself into satisfactorily producing the desired type of surface material and transporting the finished product to the point of application. The remainder of the work would consist in preparing the sub-surface with the use of modern machinery. Methods akin to railroad building might be employed with obvious improvements. Possibly an interlocking unit-surface could be supported as a bridge from side supports of poured concrete. This may appear like a dream but I venture to say that as we progress with the necessity of building a great mileage of the proper kind of roads, some such methods will be used. If such a plan were in existence, it would be possible to design a road scientifically for any given working capacity. Parts could be tested out in advance, they could be made to different specifications and the surface could be transported to the point of application with a remarkable reduction from the present ridiculously high figures for construction that are often questionable and sometimes a fraud.

Chairman Bachman:—The last two speakers have presented remarkable discussions dealing with phases of the situation that may enable us to look at our problem a little differently.

F. W. Perry:—Mr. Kennedy's remarks are rather interesting. I have been a public engineer in New York City for 25 years. The department that I represent has erected bridges, including the longest span of bridge in the world that has floors and takes care of trucks; it has built roadways of the most modern type and it has erected the New York City Municipal Building, of which I was the engineer. It was not all built in a shop, nor were the roads, nor was the bridge. Those parts that must be fabricated from the raw material are built in shops, but they must be fabricated in sections that can be handled by machinery, transported in cars or boats, lifted by derricks and placed and assembled by men. There is nothing visionary about that, but it is a difficult job. The Manhattan Bridge over the East River is today the heaviest bridge standing. It carries a congested load of 61,000 lb. per linear ft. It was designed to the last rivet in the drafting-room of our department, was delivered to New York City in pieces that were hauled up from the barges by derricks and installed in their place. That span is something like 1400 ft. in length. The Municipal Building was erected on a site that included no place to lay down a thing, not even a bag of cement. It is right in front of City Hall, surrounded by streets. A main artery, Chambers Street, was to run through the center of it. That building was erected to a height of 40 stories. Every piece had to come from the Battery, up Broadway, and was lifted directly from the truck by a derrick direct to the place where it will remain until the building comes down.

Ninety per cent of road construction lies in the foundation and the drainage. What is the use of talking about building it in a shop? We are becoming visionary. I am speaking now as a civil engineer, not as an automotive engineer. The wearing surface of a road is renewable and that can be made in a shop; wood blocks, asphalt blocks, brick, granite blocks and concrete slabs can be made there, but not in any size of section that cannot be handled on a one-man basis. Build foundations and you build roads. Drain them after you build them and they will stay there.

France has been metalling roads for hundreds of years. They run in straight lines for hundreds of miles. When the Romans came across the Rhone and went to Paris, they built the roads as straight as the crow flies. They are there yet with the old names on them, although 2 or 3 ft. deep from constant repairs. I have driven over them and, being interested in roads, took occasion to examine them. They are very steep-shouldered; the water is carried away from them and it is not permitted to get on the bed at all under the metal. The Roman roads of early history were so solid that the Roman horses had feet 1 ft. in diameter, there being no resiliency in the road; it is a matter of record. So, do not waste any time in building roads in shops.

One trouble with the road foundation bed is characteristic; it is becoming too thin. Commonly, in New York City, 6 in. of concrete is the customary foundation bed. A subway company may dig and back-fill improperly; a new roadbed will be made, the foundation structure and the smooth wearing surface will be erected on that and soon cracks develop because the structure is weak. The force of the Society's influence ought to be toward proper foundations for wearing surfaces.

A. F. Knobloch:—In regard to the thickness of the road foundation, I do not know whether Mr. Perry has any definite thickness in mind but the Society might well frame a recommendation and submit it to the Federal authorities, in view of the fact that they are now laying their plans for building a system of National highways. Mr. Perry mentioned 6 in. as being the customary thickness of a roadbed or foundation. Perhaps he has a different recommendation. This would be an opportunity to place the force of the Society's influence back of the engineering interests in the Federal Roads Board.

Weight Limitation on Roads

A Member:—I think the Society ought to initiate action in regard to weight limitation on the roads. Wisconsin recently has passed a law limiting truck units to 3½ tons. Florida has done the same thing, and this sort

of legislation is spreading all over the Country. The 7½-ton unit is a very small part of the total number. It seems to me that if we want to engineer, make and sell trucks, we must conform to public sentiment. If we do not conform to it, there will be weight-limitation legislation all over the United States very soon. The thing to do is to work out some sort of a motor-vehicle unit that will comply with the requirements and conform to public sentiment at the same time.

MR. HORINE:—As I interpret the preceding remarks on weight limitation, they propose that we cease producing heavy trucks because the public is opposed to their use on the highways. Motor-truck builders have never forced heavy-capacity trucks on the public, but have built them in response to the demand. If nobody wanted heavy trucks, no company would build them, particularly since it could sell two or more trucks of small capacity to the man who now buys a heavy truck. I would also point out that the enactments of the legislatures of such states as Wisconsin and Florida affect only country roads where the larger motor trucks very seldom run. The demand for heavy trucks is in the cities.

Even if we grant that the legislatures truly represent public sentiment on the matter of truck sizes, and that the general public does prefer to have its goods transported at higher costs by smaller vehicles, I fail to see how we are to work out a unit that will "comply with the requirements," which are for low-cost transportation, and "conform to public sentiment at the same time." If the public demands economical transport and small-capacity vehicles at the same time, the public must be educated to see the fallacy of its demands.

J. F. WINCHESTER:—A point brought out by Mr. Horine is that the 7½-ton truck gradually will disappear. He mentions that there is only a limited number of these trucks in the Country. It is a well known fact that the majority are used in and around the Eastern-coast district of the United States. After watching these and other trucks operate, there is no doubt in my mind that in the last 8 or 10 years they have done a large amount of damage to the highways. Not only has the 7½-ton truck done damage, but this is true also of the smaller units. This is brought about because, in many instances, the trucks themselves have been under-tired for their rated capacity.

The engineer designs a truck for a given load, such as 1 or 2 tons. Then he tells the salesmen the truck can carry 2½ or 3 tons. The salesmen tell their prospective customers that the truck is capable of carrying 4 to 5 tons. The truck builders and large operators of motor vehicles are reaping the results of that kind of engineering and sales work in the restrictive legislation that is being put into force today. It has been stated that there should be a load of 800 lb. per in. of tire width. That is a definite recommendation and, if the Society works along these lines, a great amount of good will be accomplished.

PNEUMATIC TIRES

We are apt to think that good roads exist and that large-size motor-vehicles operate all over the Country. This is not true. In Maryland and in many other States there are practically no large units such as Mr. Horine mentioned. The tax in some of these localities is $500 per truck for trucks of more than 5 tons, which practically precludes their use. Good roads are being built in some of the Southern and Western States. In some of these States the laws do not prohibit the use of trucks up to 7½-ton capacity. We should profit by the mistakes that have been made in more progressive States, for, if the same kind of sales work continues and is permitted in other States where good roads are being built, we will have the same kind of restrictive legislation that is being put into force in New York, New Jersey and other Eastern states.

The argument has been presented that the ultimate solution of motor-truck operation and good roads lies in the pneumatic tire. It has been stated that this is an economical piece of equipment. If compared with a solid tire, this may be true only from a standpoint of road resistance or wear. My experience leads me to believe that the pneumatic tire, particularly in large sizes for truck work, is not an economical unit in the majority of instances and, unless it is developed more in the next 5 years than it has been in the last 5 years, pneumatic tires will gradually disappear from this type of work. Their cost is prohibitive for inter-city hauling or heavy work on trucks of 2½-ton capacity and upward.

The ultimate solution of this problem will lie in the development of a cushion tire, some of which are now being manufactured under the name of semi-pneumatic. The sooner the truck builder starts to counteract the propaganda of the pneumatic-tire manufacturers, the better off he will be, as a great amount of trouble, which is handicapping the sale of trucks because pneumatic tires have been applied, is being experienced all over the Country.

The continued use of the larger units is largely a question of education. Uniform motor-vehicle laws have been proposed. They have failed in many instances because the proposed laws were supported by parties who did not take into consideration the States' or the taxpayers' interests. The result has been that the propaganda put forth by the uniform vehicle-law association is largely being discounted by the various motor-vehicle commissioners, who make recommendations to the State legislatures.

The Society should have more constructive papers of this type. It needs to impress upon the law makers that it is promoting engineering science and accumulating data which will be of value to both sides of the controversy. It may be thought by some that this paper represents the views of one or two large truck companies which produce units of 5 and 7½-ton capacity. There is no question, if careful consideration is given to all the data, many arguments are presented which should be of value in solving the problems of traffic congestion and road up-keep that daily are becoming of greater importance.

A. J. SCAIFE:—I take issue with Mr. Winchester when he says the engineer is passing out exaggerated information to the salesman. I think he will find that the salesman, by constantly coming forward and saying, in effect, "Our competitors are carrying 2000 or 5000 lb. and we must raise the weight limitations," is trying to get the engineer to commit himself.

MR. HORINE:—I think Mr. Winchester misunderstood my remarks about the number of 7½-ton trucks in comparison with those of other capacities. I did not predict their gradual disappearance, for I am confident that as we learn more about the economics of motor transport we will know more about the economy of hauling in large units, as the railroad, the steamship and the rapid-transit companies have gained more information; that the extra cost of building really durable, strong roads will be repaid many times by the lower highway-transport costs effected through the use of the most economical units. Trucks of 7½-ton capacity are not confined to

the Eastern parts of the Country, but are very popular in the Middle West and even on the Pacific Coast.

Mr. Winchester stated that trucks had damaged the roads. That is correct. Trucks do damage the roads. That is what the road is for, to be worn-out. If traffic does not wear them out, we will not build them. In the same way we might argue that tires should not be put on trucks, because they wear out. The conclusion generally drawn is that, because trucks injure roads, the heavier trucks, necessarily, do more damage. I repeat that the damage to the road is not in proportion to the capacity of the vehicle, and that possibly the damage done is due to overloading the truck or other improper operations. One form of improper operation is the practice of wearing tires too thin. It is well known that if a tire wears down to 1 in. of rubber, it might as well be a steel tire.

It is true that if this damage to the roads continues we will have more restrictive legislation. We can expect it and even welcome it, provided the restriction is against the real damager of the road and not merely an unscientific restriction in load capacity.

With regard to the pneumatic tire, I quoted from A. G. Gowans, who made the tests in California; the statements are the results of his tests. Unquestionably, the pneumatic tire has a large field. We do not know its exact limitations, and I shall not attempt to define them.

MR. WINCHESTER:—The roads are put down to be worn-out, but we must design the trucks and put tires of suitable size on them so there will be ample protection. We are apt to feel that it is a matter of road building, material and the like. The taxpayer feels that it is a combination of conditions. When the taxpayer sees a 5½ or 7½-ton truck that is greatly overloaded, occupies more than its share of the road, runs at an excessive rate of speed and sometimes causes damage through accidents to culverts and traveling roads that never were designed to carry this type of vehicle, a feeling of antagonism toward the vehicle is created.

DRIVERS AND MECHANICS

H. M. RUGG:—In reading Mr. Horine's paper I have been particularly interested in connection with the training of personnel. A study of the chart in Fig. 4 of this paper will show that to approximate the desired efficiency to any degree, the matter of trained personnel is involved in nearly all items, in the design or operation of the vehicle and in the construction and maintenance of the highway. In all discussion of service or maintenance or automotive vehicles this subject is generally acknowledged to be of importance, yet it seems to be avoided. The most flagrant disregard is in connection with the operators of the vehicles and the mechanics who keep the vehicles in repair. A trained and skilful truck operator can help to maintain highway as well as vehicle efficiency, and the trained mechanic is certainly essential to high maintenance-efficiency.

One item that might be added to the moving factor, under the heading of Vehicle Efficiency, is breakdown delays. This would cover delays on the road while the truck is in commission. The properly trained operator should be capable of making such temporary repairs as would enable him to reach his destination and possibly return to his own garage or repair-shop. This is similar to the requirements of the railroads. A locomotive engineer usually spends several years in learning to handle the machine and proving that he is reliable before he is permitted to assume the responsibility of operating it. Among the locomotive engineers it is considered almost a disgrace to be "towed-in cold," except under extraordinary conditions. However, in numerous cases we find trucks costing several thousand dollars that are operated by men who know little more than how to handle the steering-wheel and manipulate the pedals. Such a road-delay item also would emphasize the importance of properly trained men for the systematic inspection of vehicles, as well as for repair and overhauling, so that the possibility of such delays can be minimized.

I have been working for the last 3 years on the problems of training personnel. It is amazing to note the small consideration given to this subject by the manufacturers and by engineering bodies. They should be the ones most interested and should even determine the standards for conducting such training.

To keep in repair the 10,000,000 motor vehicles now registered in this Country, there should be at least 350,000 to 400,000 *skilled* mechanics. I have been criticized by manufacturers and dealers as being too conservative in the method of arriving at this figure. Present statistics show that there are approximately 45,000 automotive repair-shops in the United States, which employ about 225,000 mechanics. About 30 per cent are considered competent workmen, and a much smaller percentage than this have a knowledge of the fundamental principles of automobile design and construction. Thus, we have not over 50,000 men properly equipped to do the work for which more than 350,000 are required. It is not unusual for a man who has been washing cars or employed as a helper in one garage to work as an expert mechanic in another. If to this 350,000 men at least an equal number of trained men for properly operating trucks, more especially the larger ones, are added, the size of the educational problems can better be comprehended.

There are no standards for planning an educational program or by which the proficiency of a mechanic can be judged. This has been detrimental to the real school and unfair to its students, and permitted many schools to spring up that have practically no educational value but give the students the idea that they can become *expert* mechanics after a few weeks of attendance. Those in the engineering profession know that "experts" are not made in 6 weeks, 2 months or even 2 years. Continuous application of the knowledge acquired is necessary before a man approximates expertness.

Standards must be established so that this educational work can be properly controlled and conducted. At present the schools are between two fires, so to speak. They know that a relatively long time is required to give the proper training but, on the other hand, they attempt to meet the demands of those applying to receive it in the shortest possible time. Standards, backed by every department of the automotive industry, are the only means of solving our present educational problems. They will also help to lay the foundations of satisfactory service by supplying competent mechanics. If we accept the statement made by Norval Hawkins, of the General Motors Corporation, Detroit, that "Keeping the old cars running is more important than getting the new cars sold," we must admit the necessity of properly trained personnel.

MR. HORINE:—Mr. Rugg refers to the outline given in Fig. 4 in connection with the moving factor. He feels that breakdowns on the road should be made an element of the moving factor. The moving factor comprises only the amount of time spent in moving and in standing while in commission. All standing time, or inactivity while the vehicle is out of commission, is included under the *active* factor.

F. W. DAVIS:—Some of the important matters that are before the truck industry and also before the truck user have been discussed, but I think enough emphasis has not been placed on two items that seriously menace the industry at present, restrictive legislation and the abuse of highways.

Mr. Horine has pointed out that we are in need of a clearer understanding of the economic relation between the vehicle and the highway, but we are face to face today with the fact that there is in process of formation a great amount of legislation that will react very strongly and definitely on the development of the motor truck and its application within the next few years. I was told several months ago that sentiment existed around Boston against the use of heavy trucks because great damage had been done to some of the well known highways by the operation of heavily overloaded trucks at certain seasons of the year when these roads were ill suited to withstand such loads. This sentiment is spreading rapidly and there seems to be a feeling today in the various legislative bodies that not only must the road be protected against this abuse, but that the roads must be classified according to their carrying capacity, with certain seasonal restrictions added.

The State of Connecticut recently has enacted a law that prohibits the carrying of a load in excess of the rated capacity of the vehicle, as registered at the State Capitol. It limits the gross load that can be carried to a specified figure. The first reaction that the motor-truck companies had to this law was a very bitter opposition, a feeling that an unduly severe measure not in accord with the fundamental principles of economic transportation of materials over the highways had been passed. But, in considering the subject further, the manufacturers began to realize that, instead of being pernicious and restricting them in a way that was unfair, this law really offers the salvation to the motor-truck industry that it has been seeking for a long time. It limits the maximum load carried. It means that the violator of the law will be fined.

It is not possible to build roads throughout the Country with adequate foundations and sub-grades to carry enormous loads. Some limit must be put upon the capacities and the roads must be rated according to the means at the disposal of the road-builders. Statistics show that the truck exceeding 5½ to 7-tons in capacity is very much in the minority, possibly less than ½ of 1 per cent of all motor trucks on the highway. Fairness to the road users, who constitute the entire public, would put a sane limit on the vehicles which operate on the highways. The limitation of load per inch width of tire seems to be a very workable figure. Whether this is the ideal is entirely another question. It is determined easily and has many practical advantages.

The matter of axle loads, which are in a sense the overall weight of the vehicle, is another factor that can be determined easily. If the other States will follow the precedent set by Connecticut, a way will be opened for the truck companies to lighten their units and make them more economical in fuel and operating expense. It will also offer the real means whereby the so-called economy of operation can be increased. The example of Connecticut has been followed in almost identical form by New Jersey, and it is reported that the legislatures of New York, Massachusetts and some 12 other States are considering similar legislation.

I do not recognize the capacity of the vehicle as indicating the economy of operation. This is entirely a matter of the loads the vehicle is able to secure and carry throughout its working day. I think no one doubts the essential utility of the motor truck on the highway, but it certainly is time for the industry to exert itself in every way possible to guide legislation along desirable lines and prevent such unfortunate legislation as has been enacted in certain States. If the automotive industry will line up against excessive wheel weights of vehicles that mean the breaking-down of roads and secure the classification of highways and seasonal restrictions, I think public sentiment, instead of being against the truck that on one trip may destroy $300,000 worth of highways, will swing to the other extreme. Then we will not be forced to face the opposition that exists at present.

MR. HORINE:—Concerning the assumption of Mr. Davis that heavy trucks are responsible for the legal limitation of weight, I refer him to the results of the test highway at Pittsburg, Cal., and also to the results of the tests on impact[1] made by A. T. Goldbeck. These references ought to convince him that the impact delivered by a motor truck is not measured by its total weight, that weight is not the most important factor and that impact is not the most important factor. Mr. Davis is correct in stating that large trucks are in the minority. He intimated that it would not be detrimental to legislate them off the road. Proceeding on that basis, we would violate the fundamentals of American justice, particularly if we arraign this heavy vehicle and decide to eliminate it on a presumption that is not borne out by scientific evidence.

Mr. Davis also proposes to classify roads according to their capacity. In general, I believe that would be an excellent thing. No one will gain if the public builds a light road and a type of vehicle destructive to it is allowed to run on it. But if a State classifies its roads, specifying one road for pneumatic-tired trucks and assigning another to solid-tired trucks, and provides that on one road the speed limit shall be 9 m.p.h. and on another 15 m.p.h., the procedure must be not on a basis of the present condition of the roads but of the requirements of traffic. On a back-country road used by farmers, it would not be a hardship to restrict operation to light vehicles with pneumatic tires, because heavy vehicles would not have occasion to use that road; but on a portion of a main-traveled route that, because of improper construction, is incapable of supporting the type of traffic that would naturally flow over it such restriction would be wrong. The restriction should be placed on the highway engineer. The fate of a motor vehicle should not be narrowed down merely to the welfare of the roads. The truck should stand or fall on its economic efficiency. If the truck is advantageous to the community as a whole, it is advantageous for the community to provide a road for that truck; and it is not economic or efficient to restrict traffic on that road to the type of truck for which the road happens to be adapted. It is not economic or efficient to restrict traffic on that road to a type of vehicle that will raise hauling costs above what they should be.

Mr. Davis made a good point in saying that the economy of a truck is not measured by its capacity. The proper capacity of truck for the greatest economy is the size that most nearly meets the requirements of hauling. I hope we will not underestimate the need for heavy vehicles. A large tonnage is being hauled at present on lighter trucks than should be used. This is due to legislative restrictions, to hesitancy on the part of purchasers to invest sufficient money in a vehicle of the size they should have and to a misunderstanding of the fundamentals of operating economy.

[1] See THE JOURNAL, April, 1920, p. 265.

MR. WINCHESTER:—Mr. Davis makes the point that, because of restrictive legislation, it would be just as well to eliminate the designing of trucks of more than 5-ton capacity. It surprises me that a spirit of this kind should be in evidence when scientific data, such as are presented in Mr. Horine's paper, show that the operation of large units, which are necessary in city transportation, tends to cut down the wear-and-tear on the road-bed itself. In many instances the large unit never gets out to the State highway.

MR. HORINE:—Mr. Kennedy suggests that sometimes we get a little too close to our subject. The purpose of this paper is to persuade the automotive industry to overcome its near-sightedness. We would recommend the same process to the highway engineer. We must, as Mr. Kennedy says, get off to a greater distance. The fundamental purpose of the paper is not propaganda; it is a plea for more economic thought.

Mr. Kennedy's suggested line of 1½, 2 to 3 and 5 to 7-ton truck-capacities is really what we have today, because the capacity of the truck is not respected as it should be. The result is that every company is obliged to build its trucks, its springs and its engine with such factors of safety that the trucks can handle severe overloads; at the same time it is necessary to build vehicles having sufficient mechanical efficiency to handle economically loads that are somewhat less than the rated full capacity of the truck.

CHRONICLE AND COMMENT

(*Concluded from page* 304)

tional Automobile Chamber of Commerce has signified its willingness to support this investigation financially and by contributing personnel and equipment; some minor points in the connection remain to be settled. The American Petroleum Institute called a number of oil engineers together at a conference on April 26 to discuss the plans that had been formulated for the joint fuel-research.

After some preliminary tests were run at the Bureau of Standards showing the possibilities of securing reliable results through the comparison of different fuels in cars driven in normal service by average drivers, a supplementary program was drawn up and visits were made to a number of companies building passenger cars in the largest number, with the result that about eight different companies are running road-service tests to determine the effect of fuel volatility on fuel-consumption.

The selection of fuels for test purposes presented some rather complicated questions, as to the number of fuels to be used, the range of volatility to be covered, the limitations to be imposed as to chemical composition and as to refining methods. These questions have involved numerous conferences and visits to refineries which have resulted in the selection of four experimental fuels ranging in volatility from approximately aviation gasoline to a fuel about as bad as commercial gasoline ever gets.

The success of the automotive industry in using the fuels furnished by the petroleum industry has thus far been reasonably satisfactory, but the future holds problems more acute than those of the past. The petroleum refiners have been devising means for making more and more gasoline without undue increase in the end-point, and the automotive engineers have in turn been endeavoring to design engines that will utilize better the heavier fuels that the refiners have predicted; a process that might have been endless had its seriousness not been realized a little over a year ago.

At that time the joint obligation of the two industries was recognized in the undertaking of a joint fuel-research program that has been planned to deal, not with remote possibilities of a radical change in engine design or in fuels, but with a single definite problem affecting engines and fuels as they exist today. This program calls for an answer to the question, What Are the Relations between Fuel Volatility, Fuel-Consumption of the Average Vehicle, and Fuel Produced per Barrel of Average Crude?

A barrel of crude oil will produce more or less gasoline, depending upon its volatility. If the volatility be correctly chosen, the total ton-miles of transportation per barrel of crude will be a maximum. If unnecessarily volatile, the amount of fuel will be small and the mileage less. If too poor, while the quantity of fuel will be larger, the miles per gallon will be less and the total ton-miles will be again less than the maximum.

A systematic research program to accomplish the desired result has now been started, under the supervision of the Research Department of the Society, which department will compile the results of the tests as well.

The United States Bureau of Mines is in charge of the specifications of the fuels to be used and will make tests as to their quality, as well as control tests on fuel samples submitted by the various companies running tests.

The United States Bureau of Standards has designed and built for use in the road-testing of passenger cars representing large production, a most complete layout of indicating and recording instruments by which accurate records of fuel-consumption and all elements of car performance can be made under all conditions of road service. Thus the effect of the experimental fuels on both car mileage and car performance will be accurately known. An extensive series of road tests is being run by the Bureau of Standards, making use of fleets of Government-owned vehicles. This program will require the use of about 80,000 gallons of fuel in several hundred vehicles. Various cities have been selected in which to run the tests because of their divers road conditions.

When the different tests contemplated shall have been completed, the Research Departments of the American Petroleum Institute and of the Society of Automotive Engineers should be in a position to supply the industries they represent with information that will make possible better economic use of the petroleum resources, and longer postponement of the time when costs of fuel will retard the development of automotive transportation.

Publications of Interest *to* S. A. E. Members

In this column are given brief items regarding technical books and publications on automotive subjects. As a general rule, no attempt is made to give an exhaustive review of the books, the purpose of this section of THE JOURNAL being rather to indicate from time to time what literature relating to the automotive industry has been published with a short statement of the contents.

PHYSICAL PROPERTIES OF MOTOR FUELS. By W. R. Ormandy and E. C. Craven. Paper read before the Institution of Automobile Engineers, 28 Victoria Street, Westminster, S. W., London, England. 27 pages; 8 illustrations.

In considering the suitability of various fuels for use in commercial motor-car practice, the most important item is *not* the cost per gallon, but the *cost per unit result*. To insure economical operation a fuel must possess certain physical and chemical characteristics, which are discussed in detail in this article. Many of the data are taken from the recent publications of Ricardo and his associates.

A satisfactory fuel must have a high heat of combustion which is capable of efficient utilization. Certain substances, such as acetylene, possess high heats of combustion but cannot be used easily in the engine. It is also necessary that the fuel have a fairly low freezing-point. For this reason benzol must be mixed with some other fuel, usually a paraffin, for use in cold weather. This requires mutual solubility of the fuels used, and it has been found that certain mixtures of fuels, especially those containing alcohol, begin to separate at moderately low temperatures. Data are given for the mutual solubility of various mixtures of benzol and alcohol with other fuels.

A low initial boiling-point can be taken as an indication of the flash-point, or relative ease of starting of the fuel. A comparatively low 50 per cent point is a good indication of satisfactory behavior in the engine. High final-distillation temperatures indicate the presence of heavy fractions which possess the tendency to get by the rings into the crankcase and dilute the oil. British and Continental fuels are markedly superior to American fuels in this respect.

The latent heat of evaporation is an important factor in the carburetion of the fuel. Where a large amount of heat must be supplied it is necessary to make use of some auxiliary heating device such as the hot-spot. If a fuel with a high heat of vaporization is admitted to the cylinder in the form of spray, then the cooling of the charge and cylinder walls by evaporation makes possible a high volumetric efficiency.

Probably the most important factor in determining the starting qualities of a fuel is the amount of readily volatile material it contains. The authors have shown recently that the flash-point is capable of giving a very good idea of the amount of these volatile hydrocarbons present. The flash-points of engine gasolines are of the order of —25 deg. cent. (—13 deg. fahr.) to —30 deg. cent. (—22 deg. fahr.), of fuel benzol about —11 deg. cent. (+12 deg. fahr.), and of 95 per cent alcohol 16 deg. cent. (61 deg. fahr.). The flash-points of various hydrocarbons occur, within very small limits, at the same vapor-pressure. The lower limit of flame propagation for all hydrocarbon-air mixtures is at approximately 1½ per cent by volume of the combustible vapor, while the maximum limit is at about 4½ per cent by volume. Both the lower and higher flash-points appear to bear a constant ratio to the initial boiling-point of the fuel when all are expressed in degrees of absolute temperature.

There are several other important requirements for motor fuels; they must not exert corrosive action on the metals with which they come in contact; they must possess a fairly uniform viscosity, this is especially desirable in connection with use in jet carbureters, and they must not show too great a tendency to detonate. The detonation tendency of a fuel is indicated by its "toluene value," a notation developed by Ricardo. The authors attempt to trace a fairly definite relationship between the temperatures of spontaneous ignition and the "toluene values" of the fuel.

Referring to the development of high-efficiency high-compression engines the illuminating remark is made: "It will gradually become realized by the purchasing public that the super-efficient engine is only super-efficient when supplied with certain fuels, and that with unsuitable fuels it can become a 'super-nuisance.'" D. P. B. 4th.

THE AIRPLANE ENGINE. By Lionel S. Marks. Published by the McGraw-Hill Book Co., New York City. 454 pages; 341 illustrations.

This volume should be an important and useful addition to the literature on this subject. The author has collected and compiled in very convenient and acceptable form many of the reliable data regarding the principles of design and practical operation of all classes of typical aircraft engines in present use and has treated these data with a full regard for the underlying principles of mechanics and thermodynamics, making his interpretations of particular value.

Among other things, the volume contains well-analyzed results of tests and observations gathered from a great variety of sources, some of which are difficult of access. Naturally some of these results are impossible to verify, but the author has for the most part either omitted or qualified such statements as did not appear to be in reasonable accord with known facts or accepted theories. The author wisely makes no attempt at prediction or invention, but confines himself to what may be classed as present knowledge, presenting this in such a form as to be useful to all interested in aircraft powerplants.

AN INVESTIGATION OF THE FATIGUE OF METALS. By H. F. Moore and J. B. Kommers. University of Illinois Engineering Experiment Station. Bulletin No. 124. 185 pages; 46 illustrations.

This bulletin is in reality a progress report on a very important investigation begun several years ago under the sponsorship of the National Research Council, and with the cooperation and support of various commercial interests. Failures of steels under repeated stress have presented a difficult problem that became acute during the war. No entirely satisfactory methods of testing for this property were available. The results of tests of a large variety of steels by all the accepted and by some new methods described and carefully analyzed in this bulletin throw much new light on the subject. Some very constructive suggestions are made for the continuation of this line of research.

ABRASIVE WHEEL SAFETY CODE

A CODE containing the rules and specifications considered necessary to insure safety in the use of abrasive wheels operating at speeds in excess of 2000 surface ft. per min., which was prepared by a sectional committee organized under the joint sponsorship of the Grinding Wheel Manufacturers Association of the United States and Canada and the International Association of Industrial Accident Boards and Commissions, has been approved as a tentative American standard by the American Engineering Standards Committee. Representatives of the various engineering societies and trade associations interested in the use of grinding wheels were among the members of the committee, the Society's representative being A. J. Gifford. The sections into which the code is divided deal with types of protection devices, storage and inspection of wheels, general machine requirements, protection hoods, work rests, protection of wheels, flanges, methods of mounting, operating rules and general data. Numerous illustrations supplement the text.

ACTIVITIES OF THE SECTIONS

Secretaries of the Sections

BUFFALO SECTION—E. T. Mathewson, Acting Secretary, 321 Fidelity Building, Buffalo
CLEVELAND SECTION—E. W. Weaver, 5103 Euclid Avenue, Cleveland
DAYTON SECTION—R. B. May, Dayton Engineering Laboratories Co., Dayton, Ohio
DETROIT SECTION—B. Brede, Assistant Secretary, 1361 Book Building, Detroit
INDIANA SECTION—B. F. Kelly, Weidely Motors Co., Indianapolis
METROPOLITAN SECTION—F. E. McKone, 347 Madison Avenue, New York City
MID-WEST SECTION—T. Milton, 140 South Dearborn Street, Chicago
MINNEAPOLIS SECTION—C. T. Stevens, Reinhard Brothers Co., Minneapolis
NEW ENGLAND SECTION—H. E. Morton, B. F. Sturtevant Co., Hyde Park, Boston
PENNSYLVANIA SECTION—T. F. Cullen, Chilton Co., Market and 49th Streets, Philadelphia
WASHINGTON SECTION—B. R. Newcomb, 211 Victor Building, City of Washington

DURING the past month all of the Sections have received the annual Society appropriation of $1 for each member of the Section in good standing as of March 1. This appropriation is additional to the $500 that is sent to each Section at the beginning of the Section year, Oct. 1, by the Treasurer of the Society. The following list presents the membership totals of the 11 Sections:

Buffalo	27
Cleveland	93
Dayton	69
Detroit	303
Indiana	39
Metropolitan	197
Mid-West	82
Minneapolis	17
New England	59
Pennsylvania	63
Washington	25
Total	974

SECTIONS AND THE SUMMER MEETING

Several of the Sections have completed arrangements with the local railroad authorities for special Pullmans to transport their members to the Summer Meeting at White Sulphur Springs. In most instances these special cars will depart during the afternoon of June 19 and arrive at White Sulphur Springs on the morning of June 20 without the members having the inconvenience of any change or stopover. The Section Secretaries are being advised weekly of the members in their territory making reservations with the Society for hotel accommodations at the Summer Meeting, this service enabling the Secretaries to call the attention of these members to the advantages of traveling on the special trains. Your Section Secretary is provided with complete information and should be consulted before you purchase your transportation. A table showing the reduced fares and train schedules will be found on page 346 of this issue of THE JOURNAL. Members of every Section of the Society will profit by the reduced-fare arrangement at the 1922 meeting, the saving amounting to 25 per cent of the round-trip fare in all cases.

One of the features of the 1922 Summer Meeting will be the inter-Sectional athletic contests. The Society baseball championship series will be played to decide which Section will capture the cup now in the possession of the Metropolitan Section. There will be tug-of-war events between teams representing the various Sections. A relay race will also be contested. Word reaches us that the members of the Metropolitan Section baseball team have been perfecting their nine during the spring season, for they have no intention of having the trophy wrested from them.

It is expected that the Section stunts of previous years will be outdone at White Sulphur Springs. As announced last month, L. L. Williams is busy adding another ring to Cleveland's celebrated Burnum & Baleum exposition of the big tent which was showered with approval at West Baden. A. K. Brumbaugh is endeavoring to rejuvenate the dragon of Ottawa Beach fame. Mark Smith is studying another of the classic tongues and may burst forth at any moment in a new impersonation. Naturally the rivalry is heated and much good-natured tomfoolery will result.

SOCIETY NOMINATING COMMITTEE MEMBERS AND NEW SECTION OFFICERS

Section	*Member*	*Alternate*
Buffalo	O. J. Rohde	
Dayton	V. G. Apple	L. S. Keilholtz
Detroit	Howard A. Coffin	T. J. Litle, Jr.
Mid-West	B. S. Pfeiffer	Taliaferro Milton
Metropolitan	Cornelius T. Myers	Finley R. Porter
Pennsylvania	T. F. Cullen	F. M. Germane
Washington	W. S. James	

Each of the Sections elected its representative on the Society Nominating Committee during the past month. This Committee is charged with the selection of a ticket of general

Schedule of Sections Meetings

MAY

3—MINNEAPOLIS SECTION—Business Meeting
8—INDIANA SECTION
12—MID-WEST SECTION—Fundamental Losses in Automotive Apparatus
12—NEW ENGLAND SECTION — Manufacture of Tires—W. W. Duncan. Also Special Entertainment
18—CLEVELAND SECTION—Coordination of Automotive Research
19—DETROIT SECTION—Cylinder-Wall Surfacing Methods
25—PENNSYLVANIA SECTION—Either Outing at Torresdale or Body Meeting
BUFFALO SECTION—No Meeting Scheduled
DAYTON SECTION—No Meeting Scheduled

Society officers to be voted on for 1923, and will perform this important duty during the Summer Meeting at White Sulphur Springs. The annual Nominating Committee consists of one member of the Society elected by each Section prior to the Summer Meeting and three members-at-large who are elected at the business session of the Summer Meeting. The names of the Nominating Committee members that have been received at this date appear on the preceding page.

Each of the Sections of the Society elected its officers for the 1922-1923 year during April, the names of those who are to take office next fall having been announced at the May meetings. The complete list of these officers will be published in the June JOURNAL.

RECENT AND FUTURE MEETINGS

Detroit Section—The Detroit Section is indebted to K. L. Hermann for the arrangement of an extremely interesting and instructive production-engineering meeting on the evening of March 24. Over 225 members and guests were in attendance and heard four papers presented on the general topic of Gearing. The technical meeting was preceded by a buffet supper.

S. O. Bjornberg described several ingenious inspection and measuring fixtures in his paper, How to Use Hobs. He illustrated his talk with lantern slides and showed how the accuracy of gear hobs can be checked to assure the exact cutting of gears with teeth of the desired profile. R. S. Drummond presented a paper on the advantages of gear grinding. He called attention to the inaccuracies in tooth profile caused by the hardening process, thus demonstrating the importance of grinding the tooth face after hardening when silent running of the gears is essential. He believes that wear is greatly accelerated in the case of gear teeth that are not ground accurately to theoretical form. A paper by John Edgar treated certain features of the gear-hobbing machine that require careful adjustment to attain accurate results. He stressed the importance of using very precise hobs.

Following the presentation of the papers, a device was demonstrated that projected tooth-profiles, greatly enlarged, upon a screen and permitted comparison of the actual form with the theoretical outline.

The papers elicited very active discussion. The success of this production session indicates the advisability of the scheduling of similar meetings by the other Sections of the Society.

The April meeting of the Detroit Section was devoted to the very troublesome problem of lubricating-oil dilution in engine crankcases. William F. Parish presented the paper of the evening. He described a system that has been designed to carry the lubricant from the crankcase through an exhaust-heated distillation device to remove the diluent from the oil. The treated oil is returned to the crankcase through a cooler and the diluent collected and burned in the cylinder. Mr. Parish gave several demonstrations which added greatly to the interest in his paper.

At the Detroit Section Meeting to be held on May 19 papers will be presented on methods of producing cylinder-wall surfaces. The production problem involved is to be studied particularly from the standpoint of preventing oil-pumping and crankcase lubricating-oil dilution.

Minneapolis Section—The Minneapolis Section departed from its general practice of treating farm power and tractor topics, at its meeting held on April 5. Victor Gauvreau, instructor of mechanical engineering at the University of Minnesota, presented a paper on the Influence of Racing on Automotive Design. Mr. Gauvreau was engaged in the design of racing cars for several years. He enumerated some of the constructional improvements in car and engine design that are the result of racing experience.

The officers of the Section are making plans for a Summer Outing to be participated in by members resident in the Section territory.

Metropolitan-New England Sections—On April 21 the Metropolitan and New England Sections visited New Haven, Conn., where a joint meeting was held at the Mason Automobile Laboratory of the Sheffield Scientific School of Yale University. After a very enjoyable luncheon at the Yale Dining Club, the members inspected the laboratory apparatus, evincing particular interest in the dynamometer for measuring overall car-efficiency. Prof. E. H. Lockwood presented a very comprehensive and instructive paper on Motor-Vehicle Testing, in which he discussed many of the elements of power loss between the engine and the road. His test results and treatment of tire losses were particularly valuable, inasmuch as the effects of speed, type of tread, condition of tire with regard to wear, degree of inflation and many other variables were clearly shown.

A very novel feature of the meeting was the transportation to New Haven of 65 Metropolitan Section members by gasoline-engined rail-cars. These members traveled the entire distance via the gasoline route, the first stage of the trip being made by motorbus. The rail trip was arranged through courtesy of officials of the New York, New Haven & Hartford Railroad.

The Metropolitan Section's Annual Carnival was held on the evening of April 29. All of the plans and arrangements were made by a committee of members' wives and they set a very high standard for similar occasions in the future. Supper, dancing and entertainment drove away automotive business cares.

The New England Section has secured from W. W. Duncan a paper on the Manufacture of Tires, which will be read at its meeting scheduled for May 12.

Washington Section—E. C. Crittenden presented a paper on the Automobile Head-Lamp Problem before the Washington Section on the evening of April 7. Mr. Crittenden has been directly interested in the studies made to develop a means of measuring head-lamp illumination glare. He summarized the requirements, both legal and technical, that resulted in the most recent revision of the Illuminating Engineering Society's specifications. There was considerable discussion of the subject, it being the consensus of opinion that better regulation of head-lamp illumination is demanded. The Lighting Division of the Standards Committee of the Society will consider head-lamp intensity standards at its May meeting.

Indiana Section—The Indiana Section did not meet during April but an unusually attractive program was arranged by it for the evening of May 8. The Section will have many of the officers of the Society as its guests on this occasion, as the Council will meet in Indianapolis on the same day.

Cleveland Section—On April 21 a very interesting meeting and an inspection trip were conducted jointly by the Cleveland Section of the Society and that of the Illuminating Engineering Society. The Sections met during the afternoon at the miniature lamp factory of the National Lamp Works, where the members had an opportunity to observe the methods followed in the production of automobile-lamp bulbs. A new process of bulb manufacture was inspected later at the Pitney Glass Division of the same company. Dinner was provided at the Nela Park dining-rooms after the inspection trip, the company acting as host. Soon after dinner the Section adjourned to the lecture-room of the Nela Laboratory, where a paper on Motor Vehicle Lighting was read by H. H. Madgsick.

The Cleveland Section has secured Dr. H. C. Dickinson, manager of the Society's Research Department, to talk to it on May 18 on the Advantages of Coordinating Automotive Research.

Buffalo Section—The Buffalo Section devoted its meeting on April 18 to the topic of ball bearings. Dr. Tobias Dantzig gave a paper on the Design and Construction of Ball Bearings and Their Automotive Applications. Doctor Dantzig's paper, which was received most favorably, was followed by discussion of ball-bearing mountings and standards.

No meeting has been scheduled by the Buffalo Section to be held during May.

Pennsylvania Section—The Pennsylvania Section met on the evening of April 27. Cornelius T. Myers read a paper

(*Concluded on page* 439)

Current Standardization Work

THE Standards Committee Division meetings held during April were most successful. Several recommendations were approved for submission to the Standards Committee at the Summer Meeting at White Sulphur Springs on June 20. These recommendations are mentioned in the accompanying summary of the current standardization work and will be printed in full in the June issue of THE JOURNAL, which will contain the Reports of the Divisions to the Standards Committee. The members should therefore plan to bring the June issue of THE JOURNAL to the Summer Meeting to follow the work in the Standards Committee Sessions. Members especially interested in any of the current Division recommendations can obtain advance copies of them from the Society upon application.

The meetings held during April are listed in Table 1.

TABLE 1—APRIL MEETINGS HELD

Agricultural Power Equipment Division	April 19
Ball and Roller Bearings Division	April 28
Engine Division	April 17
Electrical Equipment Division	April 14
Electric Vehicle Division	April 5
Iron and Steel Division	April 11
Lubricants Division	April 18
Motorboat Division	April 10
Nomenclature Division	April 3
Parts and Fittings Division	April 12
Passenger-Car Hubs Subdivision	April 7
Sectional Committee on Ball Bearings	April 27
Stationary Engine Division	April 19
Thrust Ball Bearing Subdivision	April 18
Transmission Division	April 13

MAY DIVISION MEETINGS

The schedule of May meetings, published in the April issue of THE JOURNAL, has been revised slightly. The dates of the May series of meetings are therefore reprinted in Table 2. It should be appreciated that these dates are not final, but are intended to help the members of the Divisions arrange their plans far enough ahead to attend meetings. Definite meeting notices have been or will be sent to all Division members at least 2 weeks in advance of each meeting scheduled.

AUTOMOBILE WIRING

A Subdivision was appointed at the Electrical Equipment Division meeting on April 14 to formulate standards for automobile wiring based on the paper presented by W. S. Haggott at the Buffalo Section Meeting on April 21. The personnel of the Subdivision is

W. S. Haggott, chairman,	Packard Electric Co.
H. B. Burley,	Boston Insulated Wire & Cable Co.
B. M. Leece	Leece-Neville Co.
M. L. Hillmer	Remy Electric Co.
Ernest Wooler	Cleveland Automobile Co.

Consideration was given also to the standardization of cable clips and rubber bushings. As it was felt that these subjects are intimately related to that of automobile wiring, they were assigned to the same Subdivision. It was brought out that cable terminals in accordance with the present S. A. E. Standard, pages B21 and B22 of the S. A. E. HANDBOOK, are used very little and that it would be better to select terminal designs which are common practice and adopt them as standard in place of the present S. A. E. Standard.

BALL STUDS

At the meeting of the Parts and Fittings Division on April 12 the following Subdivision was appointed to revise the report on ball-studs, which was referred back for further consideration to the Division at the June, 1921, meeting of the Standards Committee:

E. R. Douglas, chairman,	Cincinnati Ball Crank Co.
H. B. Garman	Detroit Steel Products Co.
W. J. Outcalt	General Motors Co.

BRAKE-LINING

A meeting of the Brake-Lining Subdivision was held in New York City on March 28. The results obtained by several brake-lining manufacturers who had been running tests on a brake-lining testing-machine developed at the Bureau of Standards were discussed and indicated that it is very doubtful whether uniformity of results can be obtained by the use of the machine, especially when the tests are run by different operators.

The speed of the drum used by the various companies making these tests ranged from 540 to 650 r.p.m. The time required to wear the brake-linings to one-half their original thickness varied from 5 to 43 hr. The coefficients of friction determined varied from 0.420 to 0.581. The differences in results obtained, it was decided, could not be attributed

TABLE 2—TENTATIVE SCHEDULE OF DIVISION MEETINGS IN MAY

Division	Cleveland	Detroit	New York City
Aeronautic			May 15
Wire and Fittings Subdivision			May 10
Axle and Wheels	May 2[a]		
Electric Vehicle		May 10	
Lighting	May 3		
Motorboat Lighting Subdivision			May 2
Motorboat			May 23
Non-Ferrous Metals	May 1		
Parts and Fittings		May 9	
Passenger Car	May 2[a]		
Passenger-Car Body		May 8	
Screw-Threads	May 1		
Springs		May 5	
Storage Battery			May 5

[a] Joint meeting.

solely to the differences in speed, and it was brought out that the nature of the surface of the drum itself has a material effect on the life of the lining. It was concluded that it would be necessary to have the drums mounted and ground concentrically with the shaft and polished before starting the tests, but the general opinion was that the machine is too sensitive to variations of adjustment to give satisfactory uniformity of results.

A proposed modified design of testing machine was discussed and a Committee appointed to communicate with engineers of the Riehlé Bros. Testing Machine Co. to obtain their opinion as to its design.

The present S. A. E. Standard for Brake-Lining was discussed, it being considered advisable to incorporate in a general specification covering both woven and folded types of brake-lining the brake-lining dimensions specified in the present S. A. E. Standard. It was recommended that the tolerances for the width be changed from plus to minus 1/16 in. for all sizes to plus or minus 1/32 in. up to and including the 2½-in. width and plus or minus 1/16 in. for sizes over 2½-in. width. It was also recommended that the width and thickness should be measured with "go" and "no go" gages with jaws 1 in. wide.

Breaker-Contacts

At the Electrical Equipment Division meeting in April it was decided to recommend for adoption as S. A. E. Recommended Practice ¼ in. for the width across the flats of the hexagon head of breaker-contacts and of the check-nut used, and No. 10-40 or No. 8-40 thread. The adoption of this recommended practice will make it necessary to use but one size of wrench for breaker-contacts and facilitate servicing in emergencies. The diameter across the contact-points was not considered for standardization as this was thought to be a matter of individual design.

Clutch-Bearing Oiling Mechanisms

At the meeting of the Transmission Division on April 13 it was decided that the standardization of clutch-bearing oiling mechanisms is impracticable because of the great variety of individual designs that would be affected and the difficulties encountered in trying to provide an adequate standard method. This subject was therefore discontinued.

Clutch Facings

S. O. White and L. C. Fuller have been appointed members of the Subdivision on Clutch Facings to co-operate with A. C. Bryan, chairman of the Transmission Division, who is working on the further standardization of clutch facings with special reference to rivets and parallelism of facings.

In discussing different methods of testing the parallelism of clutch facings at the April Transmission Division meeting, A. W. Copland suggested that the members of the Division work up several designs of inspecting mechanisms and distribute information as to them to the Division members. Mr. Bryan felt that it might be well to establish standard methods of inspection in order to obtain uniform results from all clutch-facing manufacturers.

Clutch Standardization

The April meeting of the Transmission Division was called primarily to consider the standardization of clutches, the Society having received several suggestions in connection with this subject. C. G. Wood presented information covering trouble that the clutch and car manufacturers experience owing to the lack of standardization of clutch mountings. He referred to the tendency for the single-plate and multiple-disc clutch to replace the cone clutch and urged the Division to formulate standards for interchangeable mounting dimensions of single-plate and multiple-disc clutches. He submitted a sketch showing the possibility of such standardization, this indicating maximum dimensions to cover the most extreme cases.

General discussion indicated that the standardization of single-plate clutches is feasible, but that difficulty will be encountered in connection with the multiple-disc type. Mr. Copland suggested standardizing the connections between the driving gear and the clutch, pointing out that this is one of the important points in clutch mounting. He emphasized the importance of standardization as one of the principal economic factors in parts manufacturers' business.

Mr. Bryan brought up the matter of the advisability of standardizing the tolerances for spline fits, particularly in connection with the widths of splines on the driving-shafts and in stem gears. It was suggested that it would be well to develop standard fixtures and methods of inspecting splines to eliminate the possibility of controversy between manufacturers and consumers; also that there might be two classes of standards, one for very accurate high-grade work and the other class for less expensive work.

As it was the consensus of opinion that the subject of clutch standardization should be actively followed up, Chairman Bryan appointed the following Subdivision to investigate both single-plate and multiple-disc types of clutches and to prepare a report on their standardization:

D. E. Gamble, chairman,	Borg & Beck Co.
C. E. Swenson	Mechanics Machine Co.
C. G. Wood	Hoosier Clutch Co.
L. C. Fuller	Fuller & Sons Mfg. Co.
R. C. Merchant	Covert Gear Co.
H. W. Sweet	Brown-Lipe Co.

Mr. Bryan indicated that the Subdivision members should proceed in two groups for the present, the first three members of the Subdivision working on single-plate clutches and the last three on multiple-disc clutches. As soon as these two groups shall have obtained the necessary data and formulated a general idea of what is necessary with regard to each particular type of clutch, all the Subdivision members will confer in the preparation of the final report to the Transmission Division.

Cotter-Pins

A Subdivision was appointed at the April meeting of the Parts and Fittings Division to review the present S. A. E. Standard for Cotter-Pins, page C7 of the S. A. E. Handbook. The Subdivision appointed was

W. C. Keys, chairman,	Gabriel Mfg. Co.
W. J. Outcalt	General Motors Co.

Crankcase Drain-Plugs

At the April meeting of the Engine Division it was recommended, for consideration by the Standards Committee, that crankcase oil-drain holes shall have a minimum opening of ¾ in., be located at the lowest point in the oil-pan and be operable from under the engine hood. Owing to the large number of different engine designs and satisfactory types of oil-drain, the Division felt that further standardization would limit the engineering development of such mechanisms.

Electric Vehicle Division Meeting

Work was started in connection with the different subjects before the Electric Vehicle Division at the first meeting of the year held on April 5.

It was thought desirable to establish definite dimensions for battery trays for electric trucks so that batteries and battery assemblies can be readily interchanged. E. L. Clark and Bruce Ford were appointed to act as a Subdivision to obtain information on present practice and formulate a report for consideration at the next meeting.

A recommendation was adopted eliminating the 50-amp. charging-plug and receptacle from the present S.A.E. Standard on page B38 of the S. A. E. Handbook. This action was taken because trouble is experienced in charging-stations when using different sizes of plug and also on account of trouble caused by bad contact and resultant overheating when using the smaller plugs.

It was felt that terminal locations and polarities, as well as the types of terminal at the corners of storage-battery compartments, should be standardized. H. M. Pierce was ap-

pointed a Subdivision of one to obtain information on present practice and to prepare a report for consideration at the next meeting of the Division.

The present rather varied practice among electric-vehicle builders as to lamp voltages and sizes was discussed in connection with different State head-lamp laws. After thorough discussion, it was recommended that all incandescent lamps for electric vehicles shall be of a standard double-contact bayonet type and that for nominal 40 to 44-volt circuits incandescent lamps for tail-lamps, side-lamps and dome-lamps shall be 8-watt of the G-10 size, and those for head-lamps 15-watt of the S-11 size; and that for a nominal 80 to 84-volt batteries all lamps shall be 15-watt of the S-11 size. The poor quality of bases and sockets manufactured at present was emphasized. It was understood that there had been considerable discussion of this subject and that many improvements are being developed, which give assurance that satisfactory lamp fixtures can be obtained generally in about six months.

Emergency Rim-Clamps

The Axle and Wheels Division has voted to discontinue further consideration of emergency rim-clamps and the Subdivision, consisting of C. C. Carlton, chairman, G. L. Lavery, J. G. Swain and H. W. Kranz, has been discharged.

In 1920 the problem of formulating a means for interchanging different-size rims on motor trucks was assigned to a Subdivision of the Truck Division. A recommendation was submitted at the January, 1921, Standards Committee meeting, but referred back to the Division for further consideration. This recommendation was printed in the February, 1921, issue of THE JOURNAL. The subject was then referred to the Axle and Wheels Division, the Subdivision now discharged having been appointed in May, 1921. The Subdivision held a meeting in West Baden in 1921 in which the standardization of emergency rim-clamps was very thoroughly discussed and the decision reached that everything that had been suggested up to that time was impracticable.

Felt Specifications

Specifications for felt were approved at the April meeting of the Parts and Fittings Division for submission to the Standards Committee in June. The complete specifications cover the general classifications; character, such as "soft," "firm" or "hard"; width; weight per running yard per thickness; percentage of wool; color; tolerances and application. The principal specifications are given in Table 3. The complete specifications will be published in the June issue of THE JOURNAL.

The specifications are to be circularized among felt, axle, wheel, body and automobile manufacturers and the specifications reconsidered in the light of any comment received at the Division meeting on May 15.

Flexible Discs

The Parts and Fittings Division has tabled the subject of flexible discs, as it is of the opinion that interchanging discs and coupling spiders of different makes is not desirable and that the many methods of fastening the couplings to the discs, affecting such standardization, would introduce patent difficulties. It was also thought that it would be difficult to place responsibility in cases of dispute where parts of different makes are used, due to the different characteristics of the material.

Flywheel Housings

At the Engine Division meeting held on April 17 it was recommended that the clearance space for crankshaft flywheel bolts specified in the present S.A.E. Standards for Flywheel Housings, page A1 of the S.A.E. HANDBOOK, should be revised. The present standard specifies that the minimum diameter of the clearance space for crankshaft flywheel bolt nuts shall be 6⅛ in. and the minimum depth ⅝ in. To allow sufficient clearance when U. S. Standard nuts or S.A.E. Standard nuts with cotter-pins are used, the Division recommends that the minimum diameter of the clearance space shall be 6½ in. and the minimum depth ¾ in.

Front-Axle Hubs

At the meeting of the Passenger-Car Front-Axle Hubs Subdivision held on April 7 further progress was made on motor-truck and passenger-car front-axle hub standardization.

A tentative series of dimensions for passenger-car front-axle hubs was laid out based upon a chart showing present practice compiled by Cornelius T. Myers. Definite dimensions for the bearing layout are still to be determined, as well as for the flange and bolt-circle diameters and other detail dimensions. It is hoped that final Subdivision action can be taken in time for consideration at the Axle and Wheels Division meeting in May, but, as it is wished to have the final Division recommendation receive wide consideration, it is not planned to submit it for final action at the Standards Committee meeting next month.

The Subdivision recommended that a supplementary table, containing data on tire sizes and capacities, weights on front axles and similar data upon which the present S.A.E. Recommended Practice for Motor-Truck Front-Axle Hubs, page F1b of the S. A. E. HANDBOOK, was based, be published as an appendix to the present recommended practice. This

TABLE 3—FELT SPECIFICATIONS

Specification No.	Class	Character	Wool Percentage	Color
50-A	Pad Felt	Soft, Lofty	50 to 55	Grey and White
60-A	Pad Felt	Soft, Lofty	60 to 65	Grey and White
70-A	Pad Felt	Soft, Lofty	70 to 75	Grey and White
80-A	Pad Felt	Soft, Lofty	80 to 85	Grey and White
70-B	Pad Felt	Firm, Heavy	70 to 75	Grey, Black and White
80-B	Pad Felt	Firm, Heavy	80 to 85	Grey, Black and White
90-B	Pad Felt	Firm, Heavy	90 to 95	Grey, Black and White
95-B	Pad Felt	Firm, Heavy	95 to 100	Grey, Black and White
100-B	Pad Felt	Firm, Heavy	100	Grey, Black and White
80-C	Backcheck	Very Firm	80 to 85	Grey, Brown, Pink, Green, White
90-C	Backcheck	Very Firm	90 to 95	Grey, Brown, Pink, Green, White
95-C	Backcheck	Very Firm	95 to 100	Grey, Brown, Pink, Green, White
100-C	Backcheck	Very Firm	100	Grey, Brown, Pink, Green, White
95-D	Sheet Felt	Hard	100	Grey, White
100-D	Sheet Felt	Hard	100	Grey, White
100-E	Upper Felt	Shoe (Smooth Firm)	100	Grey, Brown, Black, Pink, Green, White
100-F	Upholstery	Smooth, Firm	100	Grey, Brown, Black, Pink, Green, White
80-G	Channel	Sole (Very Firm)	80 to 85	Grey, Black
90-G	Channel	Sole (Very Firm)	90 to 95	Grey, Black
95-G	Channel	Sole (Very Firm)	95 to 100	Grey, Black
100-G	Channel	Sole (Very Firm)	100	Grey, Black

action was taken because it is felt that the recommended practice as published at present is incomplete in that it does not explain how it was arrived at or provide any guide for the designer as to which hub and spindle size should be selected for a given application. It is also thought advisable to specify the diameter, pitch and length of thread for the threaded-end of each spindle, to make the recommended practice of real value to the manufacturers and provide a suitable means for securing uniform practice for machining axles and locking the bearings on the spindle.

The desirability of establishing a standard diameter and pitch of hub-cap thread was discussed. It was thought that, although different car-makers usually want individual hub-cap designs, a standard could be formulated that would be of benefit to the hub and wheel manufacturers without destroying the possibilities of individuality. A. S. Van Halteren was appointed a Subdivision of one to investigate the possibility of such standardization. It was also considered desirable to specify the dimensions for square-neck hub-bolts for motor-truck hubs. It was decided to consider at the next meeting adopting the standard formulated by the Wood Wheel Manufacturers Association.

Gasoline-Tank Fillers

The limiting of gasoline-tank filler-pipe openings to a minimum size was discussed at the April meeting of the Parts and Fittings Division. It was decided that passenger-car, truck and gasoline-pump manufacturers should be circularized as to their present practice in regard to the diameter of filler-pipe openings and nozzle sizes and as to their criticism of a minimum opening-diameter of 2 in.

Generator and Starting-Motor Mountings

The present standards for generator and starting-motor mountings were carefully reviewed at the meeting of the Electrical Equipment meeting on April 14, a report being submitted by the Subdivision appointed to review the standards.

The Subdivision on Generator and Starting-Motor Mountings reported that the standardization of generator strap-mountings is not feasible in view of the widely varying diameters in which generators are built and because there is no real demand for such a standard. It was therefore decided to table this subject indefinitely.

The Subdivision reported that the generator lengths between the shoulders of the shaft cannot be standardized because the essential differences in generator design require different lengths. As it was felt, however, that the same shaft-end can be standardized for both ends of such generators, the subject was referred back to the Subdivision for further consideration.

The action taken at the January Standards Committee meeting, referring back for further consideration the revision of the S.A.E. Standard for Generator-Flange Mountings, page B17 of the S.A.E. Handbook, recommended by the Electrical Equipment Division, was reviewed. It was stated that for ball-bearing installations there would be no advantage, but in fact the disadvantage of added cost for larger bearings, in changing the shaft diameter of the small size to that for the shaft diameter of the large-size flange. After discussion it was decided that no attempt should be made to standardize one shaft diameter for both sizes of generator flange.

Action was also taken towards changing dimensions *L* from 1-25/32 to 1-13/16 in. and dimension *M* from 1-15/16 to 2-1/32 in. as recommended by the Division in its report to the Standards Committee meeting last January. These changes were made so that the cotter-pin hole in the end of the shaft will provide for the standard overhang of the driving gear, a standard plain washer thickness and an S.A.E. Standard nut assembly. This subject was referred back to the Subdivision with instructions that a definite layout be made indicating the standard gear overhang and the plain washer thickness as a check on the changes proposed.

The suggestion that a flange mounting for outboard installation, but similar to the present starting-motor sleeve mounting, page B20 of the S.A.E. Handbook, should be adopted by the Society was submitted by the Subdivision. Sketches were shown of the layouts to be used with S.A.E. Standard flanges Nos. 1, 2 and 3, page B19 of the S.A.E. Handbook. It was decided to refer this entire subject back to the Subdivision with instructions to report proposed flange mountings having the same overall lengths and clearances as those shown on the present starting-motor sleeve-type mounting.

The Subdivision reported that since the S.A.E. Standard for Starting-Motor Flange Mountings, page B19 of the S.A.E. Handbook, had been changed from 13/32 to 7/16 in., a number of complaints had been received and that the Subdivision felt that an error was made in adopting the 7/16-in. diameter. General discussion indicated that the 13/32-in. hole should provide sufficient clearance for easy assembling, even in cases where the holes in the crankcase are not accurately drilled, and that the change had been made principally for tooling purposes and the selection of drill sizes in the shop. Action was taken recommending that the flange hole-diameters should be changed to agree with those originally adopted by the Society.

It was noted that in some installations it is necessary to assemble the starting motor in the flywheel case by using studs instead of bolts, because there is not room to get the bolts into the casting, but that the manufacturers do not know what length to make the studs because there is no standard thickness for the starting-motor flange. It was observed that in some generator mountings a gasket is used under the flange, but that it was not possible to design these gaskets to the same outline as the flange because some of the dimensions were lacking. It was decided to refer this matter to the Subdivision in order that a report might be formulated giving the maximum thickness for starting-motor and generator flanges and also complete dimensions for the contour of the generator flanges.

Blueprints were submitted showing a suggested mounting for airplane starting-motors used extensively at the present time by the Government and several airplane builders. It was felt that the whole situation with regard to airplane design is in such an unsettled condition that it would hardly be proper to adopt a particular type of starting-motor mounting at this time. It was therefore decided to hold this subject in abeyance until practice shall have become more established.

Ignition Distributor Mountings

The proposal to establish a definite dimension for the distance from the bottom of the Type-B distributor mounting barrel to the bottom of the tongue of the distributor half of the coupling was discussed at length at the April Electrical Equipment Division Meeting. It was felt that this dimension should be definitely established rather than leave it variable as now specified in the present S.A.E. Standard, page B16 of the S.A.E. Handbook. It was therefore recommended that this dimension be specified as 27/32 in. Changes in the bore tolerances for the distributor couplings, from 0.4930 in. maximum and 0.4920 in. minimum to 0.4915 in. maximum and 0.4905 in. minimum, were approved, as it was considered that the proposed tolerances will be satisfactory, their adoption eliminating as much looseness in this fit as possible.

Involute Splines

The subject of involute splines was tabled at the April meeting of the Parts and Fittings Division as it was felt that such standardization is not practical.

Magneto Mountings

The Electrical Equipment Division approved the Subdivision report on magneto mountings, published on page 300 of the April issue of The Journal, for submission to the Standards Committee in June as S.A.E. Recommended Practice. It was decided to include timing-lever dimensions in accordance with the present standard for motorcycle mag-

netos which specifies an advance-lever radius of 1.968 in. and a timing-lever hole-diameter of 0.2185 in.

The suggestion by L. F. Burger, of the International Harvester Co., that the flange type of magneto mountings for stationary engines should be standardized for applications where it is impossible to use the base-mounted type was discussed. The Division felt that this subject should be taken up and the following Subdivision was appointed to investigate and prepare a report on it:

A. D. T. Libby, chairman	Splitdorf Electrical Co.
W. F. Borger	International Harvester Co.
A. C. Kleckner	Webster Electric Co.
A. Rosner	American Bosch Magneto Corporation
F. L. Tubbs	Alamo Farm Light Co.

Motorboat Division Meeting

Real progress was made on several subjects at the Motorboat Division meeting on April 10, the first meeting since 1919.

It was brought out that standardization of voltage ratings would be of great assistance, present practice being such that many electrical devices are not manufactured in voltages that will permit their use on small motorboats. It was thought that the Motorboat Lighting Subdivision of the Lighting Division should consider the advisability of recommending the use of 12 volts for combination starting and lighting systems, and 32, 64 and 110 volts for separate lighting systems. The 32 and 110-volt ratings are now specified in the S.A.E. Recommended Practice for Voltage and Capacity Ratings for Isolated Electric Lighting Plants, page B37 of the S.A.E. Handbook.

It was thought that the present S.A.E. Standard for Motorboat Transmission Couplings, page E7 and E15 of the S.A.E. Handbook, should be revised, particularly in reference to the nuts necessitated by the design of certain couplings. L. Ochtman, Jr., was appointed a Subdivision of one to review these standards and to report any necessary revisions at the next meeting.

L. Ochtman, Jr., was appointed also to serve as chairman of a Subdivision to obtain information on tachometer-drive practice, as it was felt that this is a very important subject for standardization, present practice being such that charges for installing tachometers are extremely high.

The suggestion that the control levers that come as part of the engine equipment should be standardized so as to permit the use of such levers in connecting the bridge-deck controls was discussed. It was stated that present practice is such that in most cases the levers supplied by the engine builders must be thrown away as they are not suitable for the usual installations. F. B. Sexton was appointed a Subdivision of one to study this problem and report at the next meeting.

Chairman VanBlerck said that previous work of the Division showed that there was absolutely no uniformity of practice as to the height of the engine support arms from the center-line of the crankcase. It was thought that standardization should be limited entirely to medium-duty and high-speed engines for pleasure craft, inasmuch as heavy-duty engines for commercial boats are much wider and are not as a rule replaced. It was thought that any standardization should be based on the engine bore, stroke and speed, rather than the horsepower which varies to a great extent for the same bore, stroke and speed. Mr. Sexton was appointed a Subdivision to obtain information on present practice and report back at the next meeting.

It was also thought that information should be obtained as to the location of the exhaust pipe.

It was argued that the engine rotation should be standardized and the Standards Department was instructed to obtain information on present practice.

It was brought out that present practice for stuffing-boxes varies to a very great extent. It was considered that the length of the stuffing-box depends entirely upon the diameter of the shaft. William J. Deed was appointed a Subdivision to formulate a report on this subject for presentation at the next Division meeting.

The standardization of diameter tolerances for propeller shafting was discussed. It was understood that Vice-Chairman Brautigam would communicate with W. H. Bassett, of the American Brass Co., a member of the Non-Ferrous Metals Division, as to the advisability of the Non-Ferrous Metals Division formulating a specification covering propeller shafting.

Nomenclature Division Meeting

The extension of the present S.A.E. Standards for Automobile Nomenclature to cover all automotive apparatus was undertaken at the meeting of the Nomenclature Division on April 3. The automotive industry was divided into the following seven general groups as a basis upon which to work:

Aeronautics	Power Farming
Highway Transport	Stationary-Engine
Industrial Service	Operation
Motorcycling	Water Transport

These general groups were subdivided into the main types of apparatus belonging to each and the types further divided into sub-classifications. Sub-divisions were appointed to consider each of the general groups and to prepare complete tentative nomenclatures. It was understood that each Subdivision would be enlarged by the Sub-division chairman in order that qualified men might cooperate in drafting the nomenclature for the various types of apparatus.

Pipe Fittings

F. G. Whittington and W. H. Hollister have been appointed a Subdivision to review the present S.A.E. Recommended Practices for fuel and lubricating pipe fittings, for both soldered and compression types, pages C46 and C47 of the S.A.E. Handbook.

Plate Glass

The Passenger-Car Body Division Subdivision on Plate Glass is cooperating with a committee organized by the Bureau of Standards on which is represented the American Institute of Architects, the Treasury Department, the Bureau of Standards, the Glass Distributors Association, the Plate Glass Manufacturers Association, the Sash and Door Association and window and wire-glass manufacturers.

Roller Chains

At the January Standards Committee Meeting the minimum breaking-strength recommended for adoption by the Chain Division was discussed at length by several members of the Committee. This discussion was printed on page 121 of the February issue of The Journal.

The criticisms brought out in this discussion were carefully considered by the Technical Subcommittee on Chains and Sprockets, and the following points have been agreed upon by the engineers of the principal manufacturers of roller transmission chains in this country.

(1) The selection of a chain for power transmission should not be based upon the breaking strength, but upon the allowable *working load* for the conditions of the drive

(2) Designers who wish to know the approximate breaking-strength of a chain have a right to that information, and chain makers are willing to give it, *provided* they can protect themselves against those who, through ignorance, insist upon selecting their chains solely on the basis of breaking strengths

(3) The publication of a variety of strengths for chains of the same class is misleading to many buyers, and creates the impression that chain makers are vying with one another with respect to this feature that the buyers seem to regard as of first importance in the attainment of the highest quality

(4) Chain makers are therefore in favor of publishing minimum breaking-strengths, with the understanding that, although the average strengths will run much higher, they will not be guaranteed; nor are they considered necessary, since the published mini-

mum-strengths are four or five times as high as the maximum published working loads

(5) If one chain maker adds 10 or 15 per cent to the ultimate-strength of his chain without increasing the pin bearing area or the wearing quality of the parts, the allowable working load will not be increased, and his chain will not be capable of transmitting any greater horsepower. This increase in strength will generally be at the expense of light weight, or of the desirable shock-absorbing quality, or of the tight grip between assembled parts. Hence a publication of this feature will tend to mislead the innocent and unwary who are inclined to base their selection of a chain upon breaking strength alone

(6) The advance of the chain drive as an efficient and durable medium for the transmission of power requires that misconceptions be abolished, and that the attention of chain users be drawn away from the non-essentials, such as breaking-strength and polished side-plates, and fixed upon the essentials, such as working loads, speeds, number of teeth and sprocket tooth-forms

(7) The publication of uniform minimum breaking-strengths for standard chains is a step in this direction

Spring Shackle-Bolts

A Subdivision report on Spring Shackle-Bolts was submitted at the meeting of the Parts and Fittings Division in April. It is expected that this report will be finally approved at the May Division meeting and submitted for consideration at the meeting of the Standards Committee in June.

Springs Division Meeting

At the Springs Division meeting on March 14 Chairman Hess stated that it is generally felt among spring manufacturers that the S. A. E. Springs Division could do much in the way of revising existing standards and developing new ones for the spring industry. As it was thought that the best way to accomplish this is by assigning the work to Subdivisions, each Subdivision to hold meetings as frequently as necessary for the preparation of reports on the several subjects assigned to them, the following Subdivisions were appointed and subject assignments made:

SUBDIVISION A

Personnel

H. R. McMahon, chairman	Standard Steel Spring Co.
E. S. Corcoran	Kelly-Springfield Motor Truck Co.
E. V. Rippingille	Watson Stabilator Co.

Subjects Assigned[1]

Leaf-Spring Nomenclature, H1 and H2
Leaf Points, H2
Lubricating of Springs
Prime Painting of Springs
Spring Rebound Clips, Spacers and Bolts, H3
Spring Center-Bolts, H4
Finish of Springs, H2

SUBDIVISION B

Personnel

S. P. Hess, chairman	Detroit Steel Products Co.
W. M. Newkirk	William & Harvey Rowland, Inc.
R. S. Begg	Jordan Motor Car Co.
F. A. Whitten	General Motors Truck Co.

Subjects Assigned[1]

Spring Specifications, H10 to H19
Motor-Vehicle Manufacturers Specifications
Spring Manufacturers Data
Recommended Assembly Practice
Outgoing and Incoming Inspection
Flexibility Test
Capacity Test
Brinell and Transverse Bend Tests
Shrunk Center-Bands

[1] Numbers refer to S.A.E. Handbook page references.

SUBDIVISION C

Personnel

H. E. Figgie, chairman	Perfection Spring Co.
Gustof Peterson	Electric Alloy Steel Co.

Subjects Assigned[1]

Wrapped Spring-Eyes, H4
Test for Parallelism of Spring-Eyes, H5
Spring-Eye Bushing and Bolt Tolerances, H5
Width of Spring-Ends, H6
Frame Brackets for Springs, H6
Spring Offsets and Resulting Ends, H7

It was thought advisable to define adequately terms commonly used by the spring industry, in a manner similar to that followed in the Iron and Steel Standards recently adopted. It was stated that there is some confusion as to the difference in meaning between spring "deflection" and "displacement." Mr. McMahon was requested to prepare definitions for these two terms for consideration at the next meeting.

There was some discussion as to establishing a new standard for the finish of springs, but it was stated that, as the present designations cover practically all leaf-spring practice, any other kinds of finish should be considered special.

It was suggested that a standard should be formulated for practice in lubricating leaf-springs when shipped from the maker to the buyer. It was explained that some purchasers require their springs shipped lubricated, while others order them dry, and that in most cases where springs are lubricated this interferes with their painting when assembled in the vehicle. It was therefore decided not to attempt to standardize this practice, but to request the Lubricants Division to suggest which type of lubricant is best for leaf-springs.

The Division discussed at some length the desirability of specifying a standard priming-coat of paint for leaf springs inasmuch as this practice seems to be growing. It was thought that it would be practically impossible to develop a satisfactory standard specification for priming paint and that the best method of handling this subject is to provide for information on spring specification sheets as to whether or not springs should be given a priming coat.

Attention was given also to the possibility of specifying the color of the priming coat, but it was decided that this should be left to the paint manufacturer and spring maker.

It was generally agreed that a "Spring Makers' Information Sheet," on which definite complete data can be uniformly recorded for all purchasers, is very desirable. It was decided that Subdivision B should give this matter further consideration and draft such a sheet for inclusion in the S. A. E. Standard for "Spring Specifications."

As it has been found that many vehicle builders do not exercise proper care in assembling springs in vehicles, it was decided that Subdivision B should consider this subject and draft a brief set of instructions to be included under "Spring Specifications."

The troubles experienced by spring makers due to inspection methods and the rejection of springs was discussed and it was thought that definite information in this connection could be published to advantage. This subject was therefore referred to Subdivision B for study.

The present standard flexibility tests were discussed and, as it was thought they could be materially improved, the subject was referred to Subdivision B for further consideration.

Mr. McMahon stated that the subjects of Brinell and Transverse Bend Tests, which were discussed by the Iron and Steel Division some time ago, are of importance to the spring manufacturers and that he is now working closely with one of the large automobile companies in the development of such tests.

It was proposed that the threads specified for the S. A. E. Standard for Spring Center-Bolts should be changed from S. A. E. to U. S. Standard because trouble had been experienced through the stripping of the S. A. E. threads. Chairman Hess reviewed the consideration of this matter by the Division several years ago and stated that it would be a mis-

take to go back to the U. S. Standard. It was decided to refer this matter to Subdivision A for further study.

The objections raised by vehicle builders on a number of occasions to the present tolerances for parallelism were reviewed and discussed, the thought being advanced that the same tolerances should be specified for truck and passenger cars, it being largely the function of the spring maker to maintain these tolerances. It was stated that much of the trouble is due to misalignment of axles and frames, which interferes with easy assembly of springs even when the parallelism tolerances are maintained. The subject was referred to Subdivision C for an investigation of tolerances that are allowed on frames and axles and a reconsideration of the spring parallelism tolerances.

As it was felt that the S. A. E. Recommended Practice for Spring-Eye Bushing and Bolt Tolerances should not specify tolerances for any of the bolts inasmuch as these tolerances should be controlled by the bolt manufacturers in accordance with the tolerances set for the bolt-holes, it was voted to omit this part of the specification. It was stated that the present S. A. E. Recommended Practice for Spring Frame Brackets is neither complete nor uptodate and that it is logically a subject affecting the frame rather than the spring manufacturer. It was therefore decided to recommend that the present recommended practice on page H6 of the S. A. E. HANDBOOK be cancelled and to refer this subject to the Frames Division.

It was suggested that the length of rear springs for car weights of less than 2500 lb. should be increased because the stiffness of springs has decreased about 40 per cent during the past four years and longer springs are needed to obtain proper riding qualities. This subject was referred to Subdivision C for further consideration.

WIRE MESH

As information on present wire-mesh usage indicates a wide range of practice, a Subdivision of the Parts and Fittings Division has been appointed to prepare a report on its standardization. It is thought that the present range of wire sizes and meshes can be considerably reduced, and that a standard can probably be established for the kind of weave, sizes of wire and mesh, with possibly a supplementary table showing the percentage of opening per square inch for each mesh. The following Subdivision was appointed:

Clarence Carson, chairman	Dodge Bros.
E. W. Weaver	Weaver & Kemble

THE ALLIED DEBTS

IT was in the spring of 1919 that the payment of principal and interest on the Allied debts was suspended. The suspension was literally forced by the increasing gravity of the international situation. Europe has been purchasing American goods, for which she has been unable to pay except with funds from our own pocket. By private loans, the repurchase of securities held abroad and direct credits by our Government, we had advanced some $13,000,000,000 to finance these necessary purchases of our goods by Europe. When in the spring of 1919 the foreign exchanges were unpegged, and our Government ceased its advances of credit, the foreign exchanges fell heavily. Despite the violent dislocation of the exchange machinery, the extraordinary flow of goods to Europe continued, our exports in 1919 and 1920 being the largest in our history. Nothing could indicate more clearly the continued need of Europe for American help. So great has been Europe's need that her importers have had to be carried on short time by our exporters and bankers, the extent of this floating indebtedness being probably not less than $1,500,000,000 at the present time. In other words, far from Europe's being able to make interest payments on debts incurred during the war period, we have had conclusive proof of Europe's inability to pay even for her current purchases. Until this situation changes it is idle to suppose that Europe can undertake an annual payment of $550,000,000 on the Allied debt to us in addition to her payments for her current imports, interest on private loans and other obligations that have been incurred at various times.

Even if Europe could pay at this time, it is doubtful whether the American people would be prepared to accept the payment. Despite the numerous explanations given by the economists in recent years, there apparently still lingers in the public mind the fallacy that a payment from nation to nation is not materially different, in form and consequences, from a payment by one individual to another. It is a commonplace of economics that any large international payment ultimately must be made in goods. Our studies of the balance of payments of the United States have indicated conclusively that the resumption of the interest payments on the Allied debts would cause our export balance to disappear.

That the overturn of our trade balance is not in itself undesirable is amply proved by the experiences of Great Britain and Germany in the two or three generations preceding the war. Though they had a large unfavorable balance of trade, representing the earnings of their foreign investments, of the merchant marine and other sources of income, the large imports did not prevent their building up the largest export trade possessed by any nation of the world. This is a sufficient answer to the fears so commonly expressed in this country that European payments of interest, even at some future time, would flood our markets with foreign goods and ruin our export trade. Granting, however, that an eventual excess of imports need possess no terrors for us and might indeed be eminently desirable, we may well ask ourselves whether we wish to see this change occur suddenly, without proper opportunity to adjust our industry to it.—J. H. Williams in *Journal of American Bankers Association.*

ACTIVITIES OF THE SECTIONS

(*Concluded from page* 432)

entitled Some Notes on Motor Trucks. Mr. Myers treated several topics, among these being truck chassis lubrication, comparative wear of tires mounted on wood and on steel wheels, and the possibilities of rear-axle standardization. A lively and valuable discussion was had.

On May 25 the Pennsylvania Section will either hold an outing at Torresdale or a Body Engineering Meeting.

Mid-West Section—The subject of the Mid-West Section meeting of April 14 was Commercial Fuels. W. O. Hinkley, of the Peoples Gas Light & Coke Co., read the principal paper, the title of this being Actions and Results of Benzol as an Automotive Fuel. In another paper the use of alcohol as an automotive fuel was considered. The discussion following the two papers waxed hot at times but all present enjoyed the meeting and obtained a more intimate knowledge of the characteristics of motor fuels.

The results of the balloting for officers of the Section were announced, the large percentage of the Section members voting being gratifying as indicating their real interest in the Section's affairs.

At the May 12 meeting of the Mid-West Section Fundamental Losses in Automotive Apparatus will be discussed.

Applicants *for* Membership

The applications for membership received between March 18 and April 15, 1922, are given below. The members of the Society are urged to send any pertinent information with regard to those listed which the Council should have for consideration prior to their election. It is requested that such communications from members be sent promptly.

ABBEY, FRANK GORDON, student, Ohio State University, *Columbus, Ohio.*

ADAMS, CONRAD A., assistant professor of mechanical engineering, Tufts College Engineering School, *Tufts College, Mass.*

ALLEN, STANLEY C., experiment engine laboratory, Reo Motor Car Co., *Lansing, Mich.*

BACHLE, ANDREW, vice-president engineering division, Paige Detroit Motor Car Co., *Detroit.*

BAYERLINE, J. GEORGE, president and general manager, Columbia Motors Co., *Detroit.*

BERKOWITZ, SAMUEL, president and engineer, Automotive Products Corporation, *Hazleton, Pa.*

BISCHOFF, HAL C., lecturer and instructor, Sweeney Automobile and Tractor School, *Kansas City, Mo.*

BOCZ, ALEX J., mechanical engineer and designer, 2169 Richton Avenue, *Detroit.*

BOYER, RALPH L., student, Ohio State University, *Columbus, Ohio.*

BROOCK, HAROLD, 80 Longfellow Avenue, *Detroit.*

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The following applicants have qualified for admission to the Society between March 10 and April 10, 1922. The various grades of membership are indicated by (M) Member; (A) Associate Member; (J) Junior; (Aff) Affiliate; (S M) Service Member; (F M) Foreign Member; (E S) Enrolled Student.

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